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PARTICULARS AND THE RELATIONAL THEORY OF TIME

IN SPITE of the attention and dedication which ordinary language has inspired in contemporary philosophy, there is still much that may be said for the analyses embodied in various "improved" conceptual schemes. These schemes are "improved," of course, not in the sense that they expedite our everyday efforts at communication, but in the sense that they provide, or are intended to provide, some degree of clarification for philosophical puzzles and questions. One of the analyses which has proved particularly attractive to modern philosophers is the relational theory of time. Although it is not always given such a linguistic twist, we may, for our purposes, take this theory as asserting that all significant temporal expressions can, without loss in descriptive force, be replaced by untensed statements asserting various relationships of precedence and simultaneity among an appropriate set of basic existents. The relations of "precedence" and "simultaneity" are representative; there is a class of relational expressions any combination of which might prove equally satisfactory. Which relations one prefers is, to some extent, an arbitrary matter; what is important is that any temporal statement should, at least in principle, find a suitable reformulation in a tense-free idiom where relations comprise the only means of temporal determination. Such, at least, is the theory to which this paper is addressed.

I am not concerned with the claim that temporal relations alone are sufficient. There may be grounds for such concern, but I will not touch upon them here. I am, rather, interested in the sort of entities which are said to exemplify these relations—the basic existents mentioned above. There has been a tendency on the part of economy-minded relationists to identify these items with particulars; once this is done, the difference between space and time becomes primarily a difference in the relations which such particulars exemplify. The difference between space and time, on this account, is comparable to the difference between

an object being white and its being square. In contrast to this, I will argue that there is a more fundamental difference involved, a difference in the sort of entity which exemplifies these relations. Furthermore, I want to point out that this difference can be suppressed only by ignoring those objectives which originally inspired the relational theory of time. In a certain sense my argument will be directed toward taking the hyphen out of "space-time," toward separating histories from the possessors of histories, and against the notion that the machinery of tensed predication can, within the relational idiom, be replaced by something simpler—that is, something which does not exhibit a comparable degree of linguistic complexity.

I

Relational theories of time are generally elaborated within a system whose basic existents are short-lived particulars; the reasons why such ephemerality is preferred are familiar enough. There is, first, the desire to avoid an enduring subject, the "bare" particular which retains its identity through change. Second, momentary particulars have no history; at least they have no history in the same sense in which a continuant does.[1] The only event which takes place is their occurrence and this is neither followed nor preceded by any other event "belonging" to the same particular. Statements about the temporal relatedness of such entities are, therefore, free from the ambiguity which attaches to similar statements about continuants. For example, although the statement "Augustus preceded Tiberius" is ambiguous until either a specification is made or a convention adopted—usually a matter of context—about the sort of event (living, dying, reigning) common to their respective histories to which reference is being made, a comparable statement about momentary particulars is fully determinate. This, however, is not

[1] Hereafter, a "momentary particular" will signify an unchanging particular. This does not imply that it is instantaneous (zero duration), but it does mean that such a particular suffers no distinguishable alteration in its properties or relational properties. By a "continuant" I shall mean any particular which persists (i.e., retains its numerical identity) through a change in its properties and relational properties.

the most important advantage which the use of momentary particulars offers to a purely relational theory of time; if it were, the ambiguities might easily be eliminated by introducing such relations as "temporal overlap" or "wholly precedes." The other, more familiar, relations might then be defined. The third and more important consideration is that unless we deal with momentary (unchanging) particulars, the tenseless idiom of the relational theory affords no satisfactory way of describing a situation in which a particular has a property *P* and then, at a later time, does not have *P;* that is, the theory provides no consistent way of describing a particular which changes color, shape, size, or position.[2] Neither of the common methods for avoiding the contradiction involved in saying that a particular—call it a_1—has the property *P* and does not have it (at a later time) are available to the relationist; the use of tenses (for example, a_1 is *P* and a_1 was not *P*) and the use of absolute moments (for example, $P[a_1, t_1]$ and not-$P[a_1, t_2]$) introduce nonrelational temporal elements. Such considerations supply the relationist with decisive reasons for dealing exclusively with momentary particulars.

I am not particularly interested in whether one considers such particulars to be sense data, cross sections of physical objects, objective, or subjective. The remarks I have to make will apply in any case. Neither am I concerned with the terminology which different philosophers may adopt in speaking about such particulars except in so far as it might, as it often does, suggest that something else was in question besides an ordinary garden-variety particular (albeit a very short-lived one); that is, an entity (to confine ourselves to the visual characters) which has spatial position, is colored, and of a certain shape and size. Some philosophers prefer to call these "events," but this is misleading. It is misleading because events, as ordinarily understood, possess none of these properties; the Battle of Hastings, the death of Socrates, or a traffic light's turning red are neither colored, shaped, nor spatially determinate. The spatial comparisons which events do admit (for example, the traffic light's turning red occurred at the corner of Elm and Main) are plainly derivative

[2] See Gustav Bergmann's account of this matter in "Some Reflections on Time," *Meaning and Existence* (Madison, Wis., 1960), pp. 230 ff.

from those of the particulars to which they happen.[3] Events, the sort of thing we read about in history books, are primarily creatures of time; they are states of affairs, conditions, happenings, or occurrences—complexes involving a particular or particulars *being* in a certain situation or *undergoing* a specific modification. In the following pages I shall make frequent use of the term "event"; in most cases I will refer to what is commonly called a "state of affairs." Even this represents some departure from established usage since I shall include those states (*A*'s being red, *A*'s being below *B*, and so forth) which are not ordinarily considered significant enough to be called an event. Nevertheless, what we call an event depends on where our interests lie and, hence, I feel justified in appropriating this term for the "states" in which I shall presently be interested. I shall not use the term for particulars. Events, in this sense, are clearly different from particulars; the respects in which they differ have made the former unsuitable candidates for relational theories of both space and time.[4]

I do not care to deny the term "event" to those philosophers who have found it a convenient label for momentary particulars, but I do want to make clear that what they are talking about is precisely what I am talking about in speaking of momentary particulars. That many philosophers do mean this by "event" can be seen from the way they describe them: "events" are to the left or right of each other (or extend over one another spatially), they are

[3] John Wisdom, "Logical Constructions II," *Mind*, XL (1931), 462 ff.; C. D. Broad, *Examination of McTaggart's Philosophy*, Vol. I (Cambridge, 1933), pp. 142 ff.; and P. F. Strawson, *Individuals* (London, 1959), p. 57. This can also be seen by asking where, on a space-time diagram, we would locate the traffic light's turning red. Its place on the temporal axis is clear enough, but what about the remaining three dimensions?

[4] Wisdom and Broad both suggest theories in which events (processes for Broad) are the only individuals, and they both encounter difficulties in regard to space, color, shape, etc. Broad eventually talks about processes ("colourings") having color ranges, place ranges, and so on (*op. cit.*, p. 165), a supposition incompatible with his earlier statements about what can be said concerning processes. Wisdom, on the other hand, talks about two senses for his "adjoinment" relation: one for modes—a type of event—and one for particulars—constituents of modes—(*op. cit.*, p. 465 and n. p. 466; also see p. 467 for the difficulties concerning shape).

colored, and they have definite shapes and sizes.[5] The inspiration for talking about events as "small chunks of space-time" derives, of course, from relativity theory, but it is not necessary for my purpose to engage in any critique of how this concept is used in the physical sciences or how various philosophers may have adapted it to their own ends. Suffice it to say that included within the scope of this examination are those relational theories of time which have, as the terms for these relations, entities possessing the characteristics commonly ascribed to particulars (spatial relatedness, shape, and so forth). Those entities occupying small chunks of space-time fall into this class and will hereafter be referred to as momentary particulars since, quite frankly, I cannot tell them apart.

Given a system of momentary particulars, an ordering can be introduced via the precedence relation; this provides what is sometimes called the "extensive" aspect of time. The "transitory" aspect or perspectival character of our descriptions is something else again. We describe the world from a certain temporal viewpoint, and this viewpoint continually changes. Our particulars must not only be ordered, then, but we must have a means of indicating the position in such an order from which our statements are made. At this point an appeal is usually made to the distinction between naming and describing. Whatever can be directly named or referred to by a demonstrative such as "this" is, *ipso facto*, present; other particulars not so presented must be described or otherwise indicated. Moreover, of those particulars which are not directly named the ones which are future can be distinguished from those which are past or present (but not *presented*) by their appropriate relations to *this:* that is, some item which is directly named. The past (before *this*), the future (after *this*), and the present (simultaneous with *this*) are all relational properties. A slight elaboration of these ideas

[5] Even Whitehead has his "events" overlapping in space, a circumstance which implies that they have shape, size, and a certain determinate spatial location. He is apparently reluctant to admit that they are colored, but it is not altogether clear how one could determine when two events "extend over" one another (spatially) if neither is colored. Cf. *The Concept of Nature* (Ann Arbor, 1957) and *An Enquiry Concerning the Principles of Natural Knowledge* (Cambridge, 1955).

eventuates in a reformulation of the simple and complex tense structures in terms of relations alone. What we ordinarily describe as the motion or change of a continuant can then be described, at least in principle, as an appropriate sequence of temporally adjacent momentary particulars (or a series of classes of such particulars). This, in skeletel form, is the substance of most relational theories of time.

This brief sketch, even if it be considered adequate in other respects, still represents only half the story. It must be considered an analysis, I suppose, but is there any reason to think it an analysis of the temporal phenomena we actually encounter? If such a scheme is going to clarify anything, something must be said about its applicability. In order to see this point a little more clearly and to prepare the ground for what follows, consider the following exchange between a relationist and someone who we shall simply call a nonrelationist.

Nonrelationist: The scheme just outlined may, perhaps, interest the logician, but it hardly constitutes a conceptual apparatus adequate for describing the temporal features we find in our world. The theory was sketched by assuming that every particular to which reference is made is momentary, but, on the face of it, very few particulars with which I am acquainted seem to be of this sort; objects *seem* to move, change color, shape, and so forth. Even phenomenal patches, I take it, do not exhibit the type of static succession you describe.

Relationist: It is certainly true that we ordinarily describe change in this fashion (namely, in terms of an object or particular moving, changing color, and so forth), but the relational idiom constitutes an alternative way of describing these things, a way which, although not practically feasible, does contribute to philosophical clarity.

Nonrelationist: If the ordinary way of describing change represents a philosophically unsophisticated approach, then it is up to you to prescribe a different method; simply telling us that we should refer only to momentary particulars will not do the trick since, to repeat, in most cases there don't *seem* to be any. For example, in cases of continuous change (that is, noninter-

rupted transition, the type we commonly describe as a particular changing in some respect) it isn't at all clear which particular or particulars you want to talk about. We have, as we ordinarily say, only one particular, and it changes; since we cannot, according to your theory, refer to it, where are the particulars to which we can refer? I am not trying to be captious, but I certainly expect some general indication of how this scheme relates to the phenomena it purports to elucidate.

Relationist: You know, of course, that simply because you cannot isolate or distinguish such particulars does not mean they do not exist.

Nonrelationist: Neither does our failure to identify or distinguish between absolute temporal moments mean that they do not exist. Certainly you have better reasons for talking about momentary particulars than that they are necessitated by your theory of time. I don't happen to think they are necessary at all in the way you depict (see Section III), and even if they were, this would hardly be a reason for accepting the relational theory of time.

Relationist: But particulars must be momentary, whether we can actually distinguish between them or not, since a changing particular would both have a property and not have it—a contradiction.

Nonrelationist: A changing particular generates contradiction only within the relational framework. Since I am not convinced of your theory, I am still not convinced that there must be momentary particulars in terms of which you formulate your theory.

Relationist: Nevertheless, there are epistemological considerations which bear on this issue. The notion of an enduring subject, a particular which retains its numerical identity through change, is totally removed from the realm of the ascertainable. All we are ever in a position to certify is the qualitative resemblance, continuity, and so forth of those items which we experience at different times. Talk about continuants, then, is without foundation; it embodies an assumption about the permanence of the unknowable.

Nonrelationist: If this is so, where is the justification for treating every particular as momentary? If my claim that some particulars persist through time (I am quite aware that flashes and beeps may be momentary in the required sense) is metaphysically disreputable, why is your claim that no particulars persist through time in any better position? If you are right, both claims should be avoided.

Relationist: You forget that it may be possible to supplement the relational scheme with a set of instructions which make it unnecessary to deny that there are continuants or assert or assume that all particulars are momentary. We may, as Russell has often urged, not be able to deny the existence of continuants, but we can simply refuse to affirm it.[6]

Nonrelationist: And how might this be done?

Relationist: It might be done in the following way. As part of a new conceptual framework we could introduce semantical instructions or special criteria for particular identity, criteria stringent enough to preclude the identification of particulars separated by an interval of time during which change occurs. If something like this could be arranged, we would not be denying that particulars (in the old sense) persist through change; we would simply be working with an altogether new notion of "one particular." There may also be other ways of doing this.

This, then, constitutes the other half of the story of which I spoke earlier. Without some account of how these momentary particulars are to be, at least in principle, isolated and distinguished, I see nothing to recommend in a relational analysis that is not also available in other theories of time.

II

There is only one satisfactory method for delineating a system of momentary particulars in terms of which to express the relational theory. This method involves using change itself as the

[6] For example, "The Philosophy of Logical Atomism," *Logic and Language*, ed. by Robert C. Marsh (London, 1956), pp. 273-274.

criterion for generating new particulars. Ultimately, the agency used to distinguish particulars must be change since this is the only procedure capable of justifying what eventually transpires in a purely relational theory: the contradiction (owing to an exclusive use of temporal relations) inherent in the notion of a changing particular. Without first providing support for the claim that the particulars which he talks about cannot change, the inability of a relationist to consistently describe such particulars argues as strongly against the exclusive use of relations as it does against the presence of changing particulars (continuants). If, moreover, a particular cannot change, it is because change has been used, in one way or another, as the device for isolating one particular.

There have, of course, been attempts to define a system of momentary particulars in other ways. It may be thought that by referring to our actual utterances (which are, if not innpractice, perhaps in principle, momentary) we could achieve the desired fragmentation of all other particulars.[7] Other attempts rely on the notion of a "specious present" to isolate suitably short-lived particulars. There is not space to detail the objections to these procedures; in so far as they are adequate they all depend, more or less implicitly, on change itself as the final court of appeal in distinguishing particulars. This is not accidental; for, as we indicated above, this is the only way of "insuring" (without simply assuming) that the particulars in question do not change and are, thus, describable within a relational framework.

I would now like to give more precise formulation to the idea of "gearing" the generation of particulars to the change which occurs. Something like the following suggestion is, I am maintaining, the only satisfactory account the relationist can give for the alleged availability, at least in principle, of a system of momentary (unchanging) particulars.

This new proposal can be developed in the following way. A new criterion for particular identity is introduced, a criterion which precludes changing particulars. Numerical distinctions

[7] See, for instance, Donald Williams, "The Myth of Passage," *Journal of Philosophy*, XLVIII (1951), 457-472; J. J. C. Smart, "The River of Time," *Essays in Conceptual Analysis*, ed. by Antony Flew (London, 1696), pp. 213-227.

shall, according to this new criterion, be drawn wherever and whenever we have, as we ordinarily say, a particular changing properties or relations. If *A* moves, then *A* cannot be named; for the movement of *A* constitutes sufficient grounds for making a numerical distinction between the particulars which exemplify different spatial relations. *This* cannot be red at one time, green at another; rather, *this* is red and *that* is green. Within this modified conceptual scheme a particular cannot change because change has now been adopted as the agency for generating numerical differences, and it generates them at a rate which precludes any one particular from changing. It does not greatly matter whether one thinks of this as a new criterion for particular identity or as a set of instructions for the application (interpretation), in principle, of the relationist's scheme. What is important is that one realize that a new method for distinguishing particulars has been instituted, a method which guarantees the unchangeability of any particular to which reference is made. It should also be noticed that such a procedure dissolves the question "Are there continuants?" This question originally arose within a conceptual framework where both a negative and an affirmative reply were possible and neither answer, according to the empiricist's rigorous standards, justifiable. The question has now been transplanted into a context in which the affirmative answer becomes contradictory; a continuant is *a* particular which changes and is, therefore, not *one* particular.

The reason this view has attracted philosophers into maintaining a relational theory of time in which particulars serve as the fundamental terms is their preoccupation with the elements that are distinguished. If they had looked instead to those elements in terms of which the distinctions are made, an altogether different view would have emerged. If we want to know what this theory is actually employing as its basic elements in the characterization of time—what it is, in fact, appealing to in order to generate the differences between (and, therefore, the relatedness of) these momentary particulars—we must look to those elements which our criterion utilizes to effect this temporal fragmentation. As a brief glance will show, these elements are states of affairs, a succession of which constitutes the change

which occurs. The ordinary distinction between events (states) is being used to generate the extraordinary distinction between momentary particulars. Let us examine this point more closely.

Since statements can be made from any temporal perspective, we must suppose that momentary particulars can be isolated or distinguished on any occasion. In instances of continuous change, though, no such particulars are available; we ordinarily do not and, indeed, cannot (without presupposing the criterion under discussion) make the necessary numerical distinctions between successive appearances of a changing particular. In order to rectify this lacuna a new criterion for particular identity has been introduced. This criterion draws the requisite distinctions, but it does so on the basis of something other than particulars; for it instructs us to treat change as the generating agency in the delineation of particulars. An alteration in state signals the emergence of a new particular (or particulars); furthermore, any specific particular's longevity is strictly regulated by the persistence of the state in terms of which it is discriminated from its temporal neighbors. The expiration of one state of affairs (*A*'s being directly below *B*) and the appearance of another (*A*'*s* being below *B* and just slightly—just distinguishably—to the left) represent the sort of thing to which our new criterion is appealing. It is the relatedness of and distinction between these elements which enables the relationist to talk at all intelligibly about the relatedness of (and, therefore, the distinction between) unchanging particulars.

If it is color instances which supply us with the terms for spatial relations, the instancing of these colors (a particular's being of a certain color) comprises the field of the temporal relations, at least in cases of continuous change. If, as we ordinarily say, *A* changes continuously from red to green, it cannot be the instances of red and green which enable us to distinguish between that which is red and that which is green, for until something like the present criterion is introduced, these instances are the same—at least we have been given no basis for thinking them different.

We should not suppose that this criterion for particular identity can be formulated without reference, either explicit or implicit,

to the instancing of qualities (or relations); nothing else to which we might appeal exhibits the requisite temporal character. As an example of an alternative formulation, take the following. Any two particulars are identical if and only if every property which the one has the other also has. According to this criterion, then, any particular which has the property P is distinct, numerically distinct, from any particular which does not have P. In order to see the inadequacy of this formulation, consider the case where, as we commonly describe it, a particular (call it A) changes color. I will refer to the successive colors as "red," "green," and "blue," as though they were the only colors and neglect the intermediate shades which would, of course, occur in continuous change. This merely permits me to talk about familiar colors instead of utilizing a more elaborate subscript mechanism to indicate the successive distinguishable shades. If, for the moment, we call the red section of A "a_1", the green section "a_2", and the blue section "a_3", does the criterion as presently expressed achieve the distinction between these successive sections? We might suppose that a_1 is distinct from a_2 because a_1 is red and a_2 is not, and that a_1 can be distinguished from a_2 by virtue of their possession of different properties. It is clear, however, that this is entirely circular; for what is the basis for our saying that a_2 is not red? The only reason for saying this is because we presuppose that a_2 is not a_1; we presuppose, that is, that the successive sections of A are different particulars. But we should not be assuming this; our criterion was supposed to establish it. If a_1 and a_2 denote the same particular (A), a_2 *would* be red and a_1 *would* be green; they would not differ qualitatively at all, and our criterion would not distinguish between them. Therefore, unless we antecedently assume that the successive sections of A are numerically distinct, the presently formulated identity criterion is useless. Is there any reason for assuming this? Only, it would seem, if we are dedicated relationists and find ourselves without the apparatus to describe anything else. We are back in the dialectic of section I, and I do not intend to repeat it here.

The fact that red and green are, as we sometimes say, incompatible properties does not affect this matter in the least. They are

incompatible only in the sense of being instantiated by the same particular at the same time. The supposition that a_1 and a_2 denote the same particular (A), however, does not leave one particular with incompatible properties since it has them at different times. Such a situation is not precluded by our presently formulated criterion and, therefore, it will not achieve the objective (momentary particulars) for which it was introduced. If we are trying to formulate a criterion which will render a_1 and a_2 distinct, we cannot appeal solely to qualitative differences; for until we have some other method for distinguishing between them, they have all their properties in common.

If we want a criterion which actually achieves a distinction between momentary particulars (and does not simply presuppose that every particular must be momentary), we must express it differently. We can say, for example, that a numerically distinct particular is generated with each successive instantiation of a new quality (and the particular in question is that which instantiates it); or that whenever (note the temporal reference) a different quality appears (is instantiated, is exemplified) a distinct particular exemplifies it. All of these formulations, however, rely on the event or state of a particular's exemplifying a property in order to obtain the desired temporal uniqueness.

Returning once again to our original expression of this criterion (in terms of states), I think it is clear that it does, implicitly, substitute events for particulars as the terms of the temporal relations. If we are considering a change in spatial position, the momentary particular a_1 is different from the momentary particular a_2, and a_1 precedes a_2 because (the sense of this "because" will be illustrated in the next section) the state, A's being directly below B, is different, distinguishably different, from the succeeding state, A's being below and slightly to the left of B, and the particulars in question (a_1 and a_2) are confined to their respective states. Our criterion is simply a utilization of those features which states possess—distinguishability, ephemerality, and temporal relatedness—for purposes of isolating and distinguishing the appropriate particulars. Therefore, when the relationist tells us that it is statements such as "a_1 precedes a_2," where a_1 and a_2 denote momentary particulars, which constitute the

fundamental mode of temporal expression, he is mistaken—usually by his own admission. His admission consists in his appeal to a criterion similar to the one mentioned above. This criterion, in effect, stipulates that a new and numerically distinct particular is to be correlated with each successive state of affairs. Any more particulars than this would, for the questions at issue here, be unnecessary; any *less* would result in one's changing. Furthermore, each particular persists as long as, and only as long as, its corresponding state persists. The temporal character of these particulars is wholly determined by the states of affairs with which they are associated; the temporal relatedness of states determines the temporal relatedness of the particulars since it is only in terms of the former that the latter are given any temporal determination at all. To suppose otherwise would be to suppose that such particulars had a temporal identity of their own independent of the criterion used to distinguish them.

The relationist's account of time conceals what it should make manifest, and leaves implicit what it should make explicit. I say "should" because one of the cardinal virtues of the analysis, according to its exponents, its *raison d'être*, is its capacity for making explicit for purposes of philosophical clarification that which, in common parlance, is implicit. It is in the sense of being more explicit, then, that I have referred to the account in terms of events as being more fundamental.

III

For the remainder of this paper I want to do three things: (1) suggest a modification of the relational theory of time designed to meet the objections just cited, an account in which events occur, along with particulars, as basic individuals; (2) discuss some of the advantages to this procedure and some of the parallels that can be drawn with our ordinary tense structure; and (3) in section IV anticipate several criticisms.

I have been arguing that the relationist must, in cases of continuous change, acknowledge such untensed expressions as "*A*'s being *P* precedes *A*'s being *S*" as the fundamental, or more fundamental, mode of temporal expression. Such statements

depict events as the terms of the temporal relations. Ordinary language, of course, provides us, in a sense, with a precedent for this move. Except for tense, the expression "*A*'s being *P* precedes (untensed) *A*'s being *S*" is the precise analogue of the common locution "*A*'s being *P* preceded (tensed) *A*'s being *S*."[8] If we treat events as individuals, we achieve what the traditional analysis failed to achieve: a system of individuals which are mutually distinguishable and momentary. Such individuals are momentary in the required sense, for it is meaningless to suppose they endure through change. Change is the succession of different states. We could no more suppose that a given event (*A*'s being red) persists through a change in *A*'s color than we could suppose that an object moves without changing position. Neither is it necessary to appeal to additional criteria in order to distinguish events as we did in the case of momentary particulars. It should not be thought that the distinction between *A*'s being *S* and *A*'s being *P* must be drawn in terms of the characters *P* and *S;* for although these specific events happen to involve different properties, the distinction between them does not depend upon this difference. The distinction is grounded in the successive instancing (not instances) of *P* and *S*, and these, the instancing of *P* and *S*, are events, distinct events, whether the same property is involved or not.[9] A similar situation obtains with regard to space; two

[8] If this is not "ordinary" enough, we may replace it by "*A* was *P* before it was *S*." I take these two expressions to be equivalent ways of saying the same thing; both refer to events. Nevertheless, the latter, by replacing a tense-able verb by an untense-able adverb must, to retain the same temporal significance, modify the event-expression to include a means of indicating temporal perspective. That is, the event-expression (*A's* being *P*) is transformed into a statement in order to introduce the temporal determination (*A* was *P*) that can no longer be carried by the major connective. This does not, as far as I can see, alter the fact that we are still talking about the temporal relatedness of two events.

[9] For example, the particular *A* might, in changing color, be red on several occasions, but this does not mean that we have the *same* event, *A*'s being red, repeated. These events, the instancing of red, are distinguished by their occupancy of different temporal positions (i.e., one precedes the other) in the same way that identically characterized particulars might be distinguished by their occupancy of different spatial positions. If *A* is red on two occasions, we must simply refer to the two events differently: e.g., (*A*'s being red)$_1$ and (*A*'s being red)$_2$.

contemporaneous particulars (instances) may differ in color, but the numerical distinction between them is not and cannot be drawn in terms of the color properties; the distinction is made in terms of the differing instances (not instancing) of the respective colors—that is, in terms of the particulars themselves. What particulars are to space, events are to time.

I have talked about events to the exclusion of particulars because I have been primarily concerned with continuous change. It might therefore be thought that particulars have been eliminated entirely. That this is not the case can be seen from two considerations. In the first place there are flashes and bangs or their equivalents which are distinguishable and unchanging. I see no reason why events need be introduced for the purpose of describing these. Second, it is still necessary, of course, to talk about spatial relatedness, color, and shape in regard to continuous change. It should be clear by now that events do not have these properties. Since anything which does exemplify one of these nontemporal characters must not change with respect to it, we still require momentary particulars. We must therefore, in cases of continuous change, define such particulars in terms of the events of which they are constituents. It should be recalled that in doing so we shall be doing precisely what the relationist's criterion for particular identity implicitly involved. I should now like to indicate briefly a way in which this might be done explicitly.

Consider the two relations "p-part" (particular-part) and "c-part" (character-part); the former is a relation between individuals, the domain being particulars and the converse domain events. A particular and an event are related in this fashion whenever the particular is a constituent of the event; for example, A is a p-part of A's being P.[10] The latter relation stands between events (individuals) and characters (properties or relations). A character is a c-part of an event when it is a component of that event; for example, P is a c-part of A's being P. These

[10] For a given event this is a many-one relation; i. e., A or any temporal section of A is a p-part of A's being P. In other words, in saying that A is a p-part of A's being P we say nothing about the persistence of A. In the event that we have several particulars as p-parts of an event (a relational state of affairs) such as "A's being to the left of B," we would need subscripts (p_1-part, p_2-part, etc.) to indicate the specific part in question.

are explanations, not definitions. Since the particulars we wish to define are unchanging, they must endure only so long as a given state persists. Therefore, define the momentary particular a_{en} as the p-part of the event e_n which is not the p-part of any other event which either precedes or is preceded by e_n. Such a definition makes explicit everything that has hitherto been implicit. The momentary particular a_{en} precedes the momentary particular a_{em} because the state e_n precedes the state e_m and the particulars are, by definition, confined to their respective states. Particulars cannot endure through change because change is the succession of different states and, by definition, our momentary particulars cannot occupy two states which stand in the precedence relation to each other. The particular a_{en} is red if and only if redness is a c-part of e_n, and so on.

This does not mean that nontemporal statements (*A* is to the left of *B*, *A* is *red*, and so forth) are wholly derivative from statements about events. It is only when we describe the change that particulars may undergo with respect to these properties that the relationist is compelled to adopt the more elaborate mechanism just described. I want to emphasize, however, that as long as no change occurs or the change is of a noncontinuous variety, there is no reason—at least I have given none—to introduce events. It may be doubted whether there are ever situations in which no change occurs, but this objection fails to take account of the fact that we are often interested in describing "isolated" phenomena in which no change is detectable; if we enlarged the scope of our description, we would undoubtedly encounter some sort of change, but we do not always describe everything. The total state of affairs (everything which we are, in some sense, acquainted with) may change from moment to moment, but we are often interested in describing a part of this, a part in which there is no succession of different states. In these cases particulars, which in the context of more comprehensive descriptions could not be treated as unchanging, may be regarded as momentary since, with respect to what is being described, it is as if there were no temporal "passage."[11]

[11] At least there is no temporal passage in any sense in which a relationist would find difficulty.

There are several interesting corollaries to this account. One of these is the analogy which can be drawn with our ordinary tense structure. When we say, for example, that A was red, the relationist interprets this as a compound statement, a statement which can be broken down into more elementary expressions. Tenses, according to this view, simply afford us a means of making two untensed statements at once, one temporal and the other nontemporal. There is, first, the fact that a given section of A is (untensed) red and, second, the temporal fact that this section of A precedes something else which represents our temporal perspective. If the above analysis is correct, this attempt fails; we cannot, in cases of continuous change, replace the alleged "compound" statement by anything simpler. Although we can replace A by some temporal section of A—call it a_{en}—and thus, without fear of inconsistent predication, say that a_{en} is (untensed) red, this can be accomplished only by taking the complexity inherent in our mode of predication and incorporating it into the subject of that predication. That is, a_{en} already includes a qualitative determination governed by the c-part of the state (e_n) in terms of which it is defined. If this state is A's being red, then a_{en} now has, as part of its "nature," the fact that it is red—it cannot fail to be red. We eliminate a method of predication, but only at the expense of making a commensurate expansion in the meaning of our subject. As in the case of tensed predication, essentially nontemporal states of affairs become the vehicle for temporal indication.

Incidentally, this version of the relational theory reaffirms the intimate tie between time and change, a tie that is expressed most compendiously by the dictum "Change is the measure of time." Assuming that the temporal precedence relation is irreflexive and assymetrical, we cannot, on the present account, describe the "flow" or "passage" of time without also committing ourselves to change; for if "precedes" is irreflexive it must stand between different states, and the succession of different states *is* change. If the precedence relation stands between "simple" particulars, however, the "flow" of time can be described without thereby committing ourselves to any change (in the usual sense). We may have, for example, a whole system of momentary particulars

exemplifying a temporal precedence relation all of which are qualitatively identical and continuous with each other (no interruption). This would suggest, on the face of it, that we could describe temporal "passage" regardless of our acquaintance with change, and that change is an accidental concomitant to our cognition and description of temporal duration.[12] The relationist would deny this, but the reason he denies it is based on his tacit appeal to the criterion which we have attempted to make explicit. Change is the generator of the relationist's particulars, and he needs change in order to obtain two particulars which might exemplify a relation like "precedes." Unless this appeal to change is made explicit, the impression is conveyed that our description of time and our description of change are as independent as our description of an object as red and our description of that same object as square. This connection can be recovered only by reinstating events into their proper place as the terms of the temporal relations.

IV

I should now like to anticipate several objections. My remarks will be brief, but I hope they will at least indicate the general direction which a more complete answer would take.

1. Do the so-called event expressions such as "*A*'s being *S*" not contain an implicit reference to the continuant *A*? Does such an account not presuppose a system of continuants in terms of which the events are identified? In answer to this question it should be pointed out, first of all, that the sign "*A*'s being *S*" is not used to refer to *A*, nor to *S*, but to *A*'s being *S*. It should not be supposed that in referring to an event by means of this expression we also refer to the parts of this event independently. I do not deny that if *A* is (as we ordinarily say) a continuously changing particular, we will refer to successive events as "*A*'s being *S*," "*A*'s being *P*," and so forth, but this implies nothing about the identity of the respective *p*-parts of the two events

[12] I am not suggesting that temporal passage logically implies the presence of change, but I am suggesting, what I think the relationist would also maintain, that we have no way of ascertaining the former without the latter.

any more than the similarity between a_1 and a_2 (or "John" and "Joan") imply some kind of identity between their respective referents. A is a p-part of A's being S, but this is neither to say that A is a continuant or that it is identical to the p-part of A's being P. Within the present framework such statements are nonsense (see note 10). Questions about the identity of particulars are precisely the questions which events were introduced to avoid. We can now avoid such questions because we need never talk about A; we can talk about a_{en} and a_{em}, p-parts of the events which certainly are numerically distinct and nonenduring. By talking about a_{en} and a_{em} the problematic question about the persistence or nonpersistence of particulars is side-stepped; a_{en} and a_{em} are, by definition, distinct particulars, and queries about whether they are "in reality" temporal parts of the same particular are questions without sense.

It may be true, as Strawson has pointed out,[13] that the identification of events depends on the prior identification of the particulars to which they happen. Nevertheless, we can admit this (and we have by treating events as complexes, one constituent of which is a particular) and still deny that any assumption (either positive or negative) concerning the permanence of this particular is necessary. We must know, in referring to A's being S, that A is S; this implies that we have an A. All this can be admitted, however, without, so to speak, jeopardizing in any respect the temporal priority of the event. The event has parts, certainly, but the whole point in talking about the event in contrast to its parts is that it alone exhibits the temporal uniqueness and determinability essential for a relational theory of time. The parts of an event are temporally indeterminate, and the present analysis leaves them so. This account does not presuppose the prior identification of continuants because it presupposes nothing about the temporal character of an event's parts. The fact that the p-part of an event *seems* to be a continuant is an interesting, but irrelevant, circumstance in so far as the present analysis is concerned; for the only p-part of an event which we need ever talk about, the only p-part that enables us to know *what* we are

[13] *Op. cit.*, p. 57.

talking about (in this special sense of temporal persistence), is that part which certainly is not a continuant: namely, a_{en}, and so forth.

2. Granting the above, which events are relevant? When *A* is directly below *B*, it is also the case that *A* is not above *B;* why not talk about this event or state? In point of fact, an indefinite number of states might be mentioned (*A*'s not being to the right of *B*, and so on). Also, how complex can an event be? In reply to these questions it may be said that which events one talks about depends entirely upon what one is interested in describing and how finely, as it were, one wishes to draw the temporal distinctions. If *A* is moving from beneath *B*, the state "*A*'s not being above *B*" persists for a considerable length of time; *A*'s being directly below *B* does not. The latter, therefore, permits a finer or more precise delineation between successive moments—a precision that is required when we attempt a detailed description of *A*'s movement in relational terms. It may also be that by talking about more extensive events (*A*'s being between *B* and *C*) we could bisect a duration which was indivisible so long as we confined attention to less extensive states. On the other hand, there may be instances where this is not required.

3. Such expressions as "*A*'s being *S*" are descriptions; they do not denote or refer in the way a name does. Such expressions have a meaning whether they actually refer or not; as long as we know what *A* is and what *S* is, "*A*'s being *S*" is perfectly intelligible without supposing that *A* is, in fact, *S*. In response to this, I would like to know what the meaning of "*A*'s being *S*" is when no such state prevails. It certainly cannot be that *A* is *S* (or that *A* is not *S*); for although this is intelligible enough, it is false (or true), and our original expression was neither. We are not asserting anything with the expression "*A*'s being *S*," and any interpretation of its meaning which affirms that we are is simply incorrect. Neither is it incomplete in any Russellian sense. In the context of temporal statements, the only context in which we have introduced it, it remains intact. Certain propositions *about* these events (propositions expressing temporal relatedness) cannot be replaced by statements about its parts,

either the *p*-part or the *c*-part. We can certainly describe the event (for example, the state which immediately precedes *A*'s being green), but this does not eliminate the essential reference to events. No other descriptive phrase not involving a reference to events, will do. We cannot, for instance, talk about "the temporal section of *A* which is red" or "the temporal section of *A* which is red and no other color"; for this involves a reference to the particular *A* which we are (see section I) trying to avoid.[14] Besides, even if this were satisfactory in other respects, it would leave us with no way of indicating temporal perspective—nothing of sufficiently short duration is being named in contrast to being described. The fact that an ascriptive expression is implied by successfully referring to a given state of affairs should not induce one to think that what is actually being done in referring to it is describing it. The truth of the matter is that in referring to a given state all the conditions are met for truthfully asserting a given proposition. We may, of course, want to eliminate the gerundial form (*A*'s being red) which I have consistently used. This is unimportant; what is important to remember is that in any explict presentation of the relational theory of time, (a) some such terms are indispensable, and (b) they do refer to states of affairs.

4. Finally, does such a system not violate the strictures of type theory? An event is of zero level, but one of its parts, the *c*-part, is of type 1. Does this not mean that qualities are operating on two different type levels? Also, of what type is the *c*-part relation?[15] I admit that the *c*-part relation is heterogeneous, but this simply means, as far as I can see, that we cannot talk about its field or type. Its domain is of type 1 and its converse domain of type 0. We may, if we like, call it a relation of second level, determined by the argument of highest type. As far as the qualities which occur as parts of events go, I fail to see why we must

[14] We might try "the particular which is red and of no other color," but until we introduce the criterion (formulated in terms of events) already discussed, nothing satisfies this description; to use our previous example, *A* is red *and* blue.

[15] This objection was suggested to me by Professor Julius Weinberg. He is not responsible, however, for the way I have stated it.

consider a quality as of the same type as the event of which it is a component. The event is not a class consisting of a particular and a quality (or relation); it is a particular's having this quality or standing in this relation. Hence, I see no reason why the event cannot be of type 0 while one of its parts (this is, perhaps, an unfortunate word) is of type 1—any more than I can see why a machine must be light because one of its parts is light.

These rather cursory remarks may fail to satisfy, and they may, I suspect, even raise more doubts than they settle. If this be true, I can do no more than point out that something along these lines seems to be required if my earlier arguments are sound. Assuming they are, there appear to be very few alternatives which remain open. We have just suggested one of them, but some might be willing, perhaps, to sacrifice this explicitness in favor of an ontological predilection. Or we might admit the necessity for events but deny the sort of position just sketched. It may be urged that the conceptual foundations for our spatiotemporal discourse has no fundamental level. Instead, such discourse is constituted by an amorphous mass of interconnected concepts, none of which can be considered as any more fundamental than the rest. This is an alternative which many would undoubtedly find inviting, but a discussion of its implications is beyond the limits of this essay. It may not be a wholly inaccurate generalization, however, to say that anyone of such a conviction would also sympathize with our last alternative: namely, an abandonment of the relational program altogether. If we do not take this step, however, I am convinced that something resembling the approach sketched here represents the only line of development consistent with the analytical intent of relational theories of time.

FRED I. DRETSKE

University of Wisconsin

ARISTOTLE ON SENSE PERCEPTION*

IN MODERN times some kind of distinction has generally been made between the physiological condition of perception—an event or series of events in the body—and the psychological or mental event of perception itself, which used to be said to occur in the soul. This distinction goes back at least to Descartes, and modern commentators generally read it into Aristotle's *De Anima* (hereafter referred to as *DA*). Thus G. Rodier understands Aristotle as holding that the modification of the sense organ is only the "condition" of perception.[1] R. D. Hicks thinks that Aristotle distinguishes soul from body and holds that perception occurs in the soul, not the body.[2] W. D. Ross realizes that Aristotle does not make a clear distinction between physiological conditions and mental events, but Ross sees him as working toward it.[3] Thus the most interesting thing about Aristotle's discussion of perception is obscured and even ignored: Aristotle tries to explain perception simply as an event in the sense organs.

This paper analyzes that attempted explanation in detail, with particular attention to the claims that sense is a *mesotes*, or "mean" (424a 4), and that one perceiving becomes "like" the things he perceives (418a 4-6).[4] My analysis shows why these claims seem to have explanatory value, and why they in fact have none. It also shows why Aristotle conceived of intellect as something nonbodily (*DA*, III, 4).

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* This article is drawn from a dissertation produced at Cornell University under the direction of Professor David Sachs.

[1] *Aristote Traité de l'âme* (2 vols.; Paris, 1900), II, 261, 330.

[2] *Aristotle De Anima* (Cambridge, 1907), pp. xlviii, 483.

[3] *Aristotle* (5th ed. rev.; London, 1953), pp. 136-137.

[4] Aristotle's word for "like" is οἷον. The Oxford translator renders this word as "identical in quality with." Unless otherwise indicated, quotations of Aristotle in English are from *The Works of Aristotle Translated into English* (12 vols.; Oxford, 1931). For the most part I quote from the reprint of these translations in *The Basic Works of Aristotle*, ed. by Richard McKeon (New York, 1941). Quotations of the Greek text of the *De Anima* and the *Parva Naturalia* are from the Loeb Classical Library edition by W. S. Hett (London, 1957).

I

Aristotle opens his discussion of perception in *DA* II, 5, by describing perception as a kind of "change" or "being acted upon" (416b 33-34).[5] It becomes clear that what Aristotle has in mind here is the change which external objects produce on the various sense organs. Thus he says that "the objects that excite the sensory powers to activity, the seen, the heard, etc., are outside" (417b 20-21), and he goes on in *DA* II, 7-11, to explain how each of the sense organs is acted upon by a different sort of external object. The bulk of his discussion of perception is directed toward showing that each kind of object affects a sense organ via some medium. Thus color causes change in something transparent, such as air, which in turn causes change in the eye (419a 14-15). A sounding object sets in motion a mass of air, and this in turn affects the organ of hearing (420a 3). Odors affect the organ of smell via air or water (421b 8). Even flavored and tangible objects affect the organs of taste and touch via a medium, namely the surface flesh. The surface flesh is not itself the organ of taste or touch; rather, these organs are located somewhere within the flesh (422b 33 – 423b 26).

At the end of this long account of the way in which external objects affect the sense organs, Aristotle tries in a brief but crucial passage to explain what sense is and why it has the power of discerning (423b 27 – 424a 9). His theory is developed from a consideration of touch, but it applies to perception generally (423b 33). It is this passage which I wish to examine in detail.

> What can be touched are distinctive qualities of body *as* body; by such differences I mean those which characterize the elements, viz. hot cold, dry moist, of which we have spoken earlier in our treatise on the elements [423b 27-30].

In the "treatise on the elements," that is, the *De Generatione et Corruptione*, Aristotle argues that the other pairs of tangible

[5] ἡ δ' αἴσθησις ἐν τῷ κινεῖσθαί τε καὶ πάσχειν συμβαίνει. In the Oxford translation: "Sensation depends ... on a process of movement or affection from without." Whether perception "depends on" this change, as the Oxford translator reads it, or *is* this change, as I suggest, will be settled below. Aristotle's use of the phrase συμβαίνει ἐν does not decide this issue. Hett, in the Loeb edition, and Hicks, *op. cit.*, both render this phrase as "consists in."

qualities—heavy-light, hard-soft, viscous-brittle, rough-smooth, and coarse-fine—derive from the two primary pairs: hot-cold and dry-moist (*De Gen.* II, 2).

> The organ for the perception of these is that of touch—that part of the body in which primarily the sense of touch resides. This is that part which is potentially such as its object is actually: for [all] sense perception is a process of being [so] affected; so that that which makes something such as it itself actually is makes the other such because the other is already potentially such [423b 30–424a 2].[6]

The "these" in the first line refers to the tangible qualities hot-cold and dry-moist mentioned above. By saying that the sense of touch "primarily" resides in the organ, Aristotle means only to contrast the organ of touch with the surface flesh, the medium of touch, in which the sense of touch does not primarily reside.[7]

The first important point in these lines is that Aristotle here seems to speak interchangeably of perception and of the action by the object of touch on the organ. The organ, a "part of the body," is said to be "potentially such as its object is actually," and the object is said to make the organ "such as it itself actually is." Perception seems to be simply that "process of being [so] affected." So interpreted, perception is simply the movement which occurs in the sense organs, not some psychic process in addition to the movement in the organs. This interpretation will be substantiated in what follows.

The second important point is that Aristotle here states a hypothesis about perception which he thinks has explanatory value, namely the hypothesis that perception is a process in which the object perceived makes the sense organ "such as it itself actually is." To understand what this hypothesis means, let us see how Aristotle uses it in lines immediately following the above to explain certain phenomena of perception.

> That is why when an object of touch is equally hot and cold or hard

[6] I have bracketed the words "all" and "so" in the Oxford translation, since they are not explicit in the Greek: *τὸ γὰρ αἰσθάνεσθαι πάσχειν τι ἐστίν.*

[7] Hicks points this out, citing 422b 22 in support (*op. cit.*, p. 412).

and soft we cannot perceive; what we perceive must have a degree of the sensible quality lying beyond the neutral point [424a 2-4].

The word "equally" in the statement that we cannot perceive what is "equally" hot and cold or hard and soft must mean equally hot and cold or hard and soft as compared with ourselves or with some part of ourselves. Since it is the sense organs which are in question here and which are said to change from being potentially such as the perceived objects to being actually such as the perceived objects, the "equally" hot and cold or hard and soft must mean as compared with the sense organs. The sense organ involved in perceptions of hot and cold, hard and soft, is the organ of touch. Thus, Aristotle here makes the following factual claims: we cannot perceive by touch a temperature or a hardness equal to that of the organ of touch; what we perceive by touch as hot or cold must be hotter or colder than the organ of touch; what we perceive by touch as hard or soft must be harder or softer than the organ of touch.

Assuming that these factual claims are correct, and given that Aristotle's hypothesis about perception is supposed to explain "why," διό (424a 2), these facts occur, what is the meaning of this hypothesis? It is expressed in the statement that sense perception is a process in which the perceived object makes the sense organ "such as it itself actually is." If this statement be taken literally, it means that perception is a process in which perceived hot objects make the organ of touch hot, and so on. This literal interpretation is exactly what we must give to the statement if it is to explain "why" we cannot perceive what is equally hot and cold or hard and soft. For what is hot or cold or hard or soft in a certain degree cannot *change* to being hot or cold or hard or soft in that degree. And if perception is a process in which the sense organ must *become x* when *x* is perceived, then we would not be able to perceive what is equally *x*.

So far Aristotle's theory is stated only for perceptions of hot and cold, hard and soft. But it is applied to perception generally (423b 33), and a few lines later it is explicitly stated for perceptions of color:

As what is to perceive *both* white and black must, to begin with, be

actually neither but potentially either (and so with [all] the other [sense-organs]), so the organ of touch must be neither hot nor cold [424a 8-9].[8]

Just as the organ of touch must not be equal in temperature to the thing perceived as hot so that it can *change* to hot, so the organ of sight must be neither white nor black so that it can change to white or black. Aristotle's reasoning can only be that perception of white is a process in which the organ of sight becomes white, and so on for the perception of the other colors. The same conclusion is extended *mutatis mutandis* to the other sense organs.

There is a striking passage in *DA* III which confirms this interpretation. Aristotle raises the question as to how we are aware that we are, for example, seeing. He says that if we are to be aware that we are seeing by a kind of sight, "then if we are to *see* that which sees, that which sees originally must be coloured" (425b 18-19). But this offers no difficulty for Aristotle because "in a sense even that which sees *is* coloured; for in each case the sense organ is capable of receiving the sensible object without its matter" (425b 23-24). Aristotle says that that which sees is colored, which seems to mean that the sense organ is colored. While the "in a sense" makes Aristotle's meaning vague, he has to hold, if he is to solve the present difficulty by this argument, that the organ becomes colored in the act of perception in a way in which it could be *seen*. Hence it seems that the organ must become colored in the sense in which it could be said to become red, green, and so forth. Furthermore, while Aristotle expressly discusses here only the case of seeing, the difficulty he considers applies generally to all the senses, namely, how we are aware that we are perceiving. Presumably Aristotle intends that his solution for the case of seeing be extended to all the other senses.

I therefore submit as conclusively established a literal interpretation of Aristotle's statement that perception is a process in which the perceived object makes the sense organ "such as it itself actually is." This interpretation can be expressed in the

[8] I have bracketed the words "all" and "sense-organs" in the Oxford translation, since they are not explicit in the Greek: *οὕτω δὴ καὶ ἐπὶ τῶν ἄλλων.*

following proposition, which I will label *A: an object which is perceived to be* x *makes the sense organ involved in its perception to be itself* x.

Ross agrees that Aristotle holds *A*, though he regards it as an incidental slip on Aristotle's part.[9] The important question is whether Aristotle holds (i) that perception is *identical* with the organ becoming *x*, identifying, for example, someone's seeing a red object with his organ of sight becoming red; or (ii) that sense perception *depends on* the process in which the organ becomes *x*, but is not identical with it. This question can be settled by returning to the passage which we have been studying. The conclusion that we cannot perceive what is equally hot and cold or hard and soft depends on the claim (*A*), that when something is perceived to be *x*, a sense organ becomes *x*. It is a *sense organ* which is said to become *x* when *x* is perceived, as I have shown, and it is what is equally hot and cold or hard and soft with the *organ* of touch which we cannot perceive.

From these alleged facts about the *sense organ*, Aristotle now goes on to explain what *sense* is.

> This implies that the sense [itself] is a "mean" *[mesotes]* between any two opposite qualities which determine the field of that sense. It is to this that it owes its power of discerning the objects in that field. What is "in the middle" is fitted to discern; relatively to either extreme it can put itself in the place of the other [424a 4-7].[10]

Aristotle here tries to explain what sense is and why it has the power of discerning. This explanation is based on the concept of sense as a "mean" or midpoint between extremes (the literal meaning of *mesotes*). For Aristotle each sense covers one or more ranges of sensible qualities between extremes. For example, sight covers colors, which range between the extremes of black and white (see 426b 8-10). The power of sense—that is, the power to discern—is explained simply by the fact that each sense is a "mean" between the extreme qualities which determine the range of that sense. Apparently to say that the sense is a "mean" is

[9] *Loc. cit.*

[10] I have bracketed the word "itself" in the Oxford translation. Aristotle says simply ἡ αἴσθησις.

to say that sense can move toward one extreme or the other or, as J. A. Smith puts it in the Oxford translation, "relatively to either extreme it can put itself in the place of the other."[11]

From the claim that we cannot perceive what is equally hot and cold or hard and soft with the *sense organ*, Aristotle thus concludes that the *sense* is a "mean" between the extremes. He thus apparently holds that the movement toward extremes which the *sense* is said to accomplish is identical with the change to hot or cold, hard or soft, which occurs in the sense organ. On this interpretation the relation between the sense and the sense organ would be as follows: the *sense organ* is a part of the body which becomes hot when heat is perceived, and so forth; each *sense* is located in a part of the body and is the *power* of the sense organ to change in temperature or color, and so forth; on this interpretation, the power to discern temperature is explained simply as the power of the sense organ to change in temperature; the power to discern color is explained simply as the power of the sense organ to change color, and so on for the other senses. There is no distinction between the change of the sense and the change of the sense organ. The only distinction between "sense" and "sense organ" is that the word "sense" refers to the power of the sense organ to change within a certain range of qualities.

If we do not interpret Aristotle in this way, then his explanation of the power of discerning, which sense has, by the claim that sense is a "mean" in no way follows from his claim that when something is perceived to be *x*, a sense organ literally becomes *x*,

[11] It is true that the last line of this passage is far from clear in the Greek. Smith's "relatively to either extreme it can put itself in the place of the other" renders *γίνεται γὰρ πρὸς ἑκάτερον αὐτῶν θάτερον τῶν ἄκρων*, which might be more literally rendered as "for it (the mean, i.e. the sense) becomes other (*thateron*) toward each of the extremes." But the word *thateron* is frequently used in expressions indicating alternation from one way to another or from one side to another. Hence in the context of this passage, Smith takes *thateron* as meaning that the sense can move in one or the other direction in a range between extremes. Hicks (*op. cit.*) reads the line as saying that the mean, i. e. the sense, becomes *opposite* to each extreme, but it is hard to make sense out of this: "For the mean is capable of judging, becoming to each extreme in turn its opposite." I follow Smith's translation, according to which Aristotle means that the sense can change toward either extreme in a range of sensible qualities.

and from his use of this claim to explain why we cannot perceive what is equally hot and cold. But Aristotle claims that his explanation of sense does follow from these claims about the sense organs, as is shown by the way in which he connects the two ideas in 424a 4, analyzed above. Therefore I conclude that in this passage Aristotle holds that the perception of *x* is identical with a sense organ becoming *x*, because he tries to explain the perception of *x* as the change of the sense organ to *x*. My interpretation can be expressed in the following proposition which I shall label *B: the perception of* x *can be understood as a sense organ becoming* x.

There is an obvious difficulty for such a theory of perception, a difficulty which Aristotle makes explicit in *DA* II, 12, almost immediately following the passage I have analyzed above. He notes that plants, for example, do become hot and yet do not perceive heat (424a 32-34). Why not? The fact that Aristotle raises this question does not overturn my interpretation of him by *B*. Indeed this question would naturally arise given the theory of perception expressed by *B*. In answering it, he does not reject the view that perception is a becoming *x* in a sense organ, but tries to show that a sense organ becomes *x* in a special way.

Thus he says of plants:

> They have no mean [of contrary qualities], and so no principle in them capable of taking on the forms of sensible objects without their matter; in the case of plants the affection is an affection by form and matter together [424b 1-3].[12]

Aristotle says that plants cannot perceive because they do not have a *mesotes* or "mean" and hence cannot receive "forms of sensible objects without their matter." I will turn to the latter phrase in a moment. First, let us consider the claim that plants do not have a *mesotes*. As this term was introduced in 423b 27–424a 9, it meant the power to become hot, red, and so forth. We were able to understand Aristotle's concept of sense as a *mesotes* because we took it as meaning that a sense organ becomes *x* in the same way that, for example, a plant becomes *x*, and we

[12] I have bracketed the phrase "of contrary qualities" in the Oxford translation, since it is not explicit in the Greek.

know what it is like for a plant to become *x*. But then the concept of sense as a *mesotes* cannot be used to explain how the becoming *x* of a sense organ *differs* from the becoming *x* of a plant.

In the rest of *DA* II, 12, Aristotle continues his attempt to explain the difference between beings which can perceive and beings which cannot. Yet it is extremely interesting that after 424b 3 he does not use any of the philosophical language with which he has described perception, such as the claim that sense is a "mean." Instead he relies on a word in ordinary use, *aisthanesthai*, meaning "perceive" or "observe" or sometimes, more specifically, to "see," "hear," or "feel." Thus he notes that air cannot smell odor, though it does become odorous (424b 15-16). He goes on:

> But smelling is more than such an affection by what is odorous—*what* more? Is not the answer that, while the air owing to the momentary duration of the action upon it of what is odorous does itself become perceptible to the sense of smell, smelling is an *observing [aisthanesthai]* of the result produced? [424b 16-19].

But what has he explained here? Smelling differs from becoming odorous in that smelling is an "observing" or "perceiving." But if asked what "observing" is, Aristotle would have to reply that observing is, for example, smelling. He has merely replaced the specific term "smell" by the generic term "observe." He is trying to explain how perceiving *x* differs from becoming *x*. If the answer is put in generic terms it becomes a tautology: perceiving *x* differs from becoming *x* in that perceiving *x* is perceiving *x*.

Thus Aristotle's attempt to explain perception fails. He begins with the conception of perceiving *x* as a kind of becoming *x*, and of sense as a "mean" or power to become *x* within a certain range. He realizes that perceiving *x* must be a special kind of becoming *x*, yet when he tries to show what is special about it, he finds himself, forced to say simply that perceiving is perceiving.

By using the word *aisthanesthai*, "perceive," it is possible to give content to the statement that sense is a *mesotes:* it means simply that sense is the power to perceive. But then the technical term *mesotes* depends for its own meaning on the word "perceive" and adds nothing to it. It cannot be used to *explain* what it is to perceive.

Let us now return to the claim that plants cannot perceive because they cannot receive "the forms of sensible objects without their matter" (424b 2). Here also Aristotle tries to distinguish the becoming *x* of the sense organ from, for example, the becoming *x* of a plant.[13] A plant which becomes hot would be said to "receive a form," according to the general theory of change in *Physics* III, 1-3. The sense organs receive the form of heat in a *special* way, "without matter." We can understand this if it means simply that one "sees" or "perceives" heat, but then the phrase "receiving forms without matter" cannot be used to explain what perception is; it simply repeats the meaning of "perceives" in obscure philosophical language.

Aristotle makes one other attempt to distinguish the becoming *x* of a sense organ from the becoming *x* of other things, namely in his contrast between the two senses of the term "alteration" or "change of quality" (*DA* II, 5).

Ordinary changes of quality involve the loss of one quality when another is gained (417a 32; 417b 3, 16), whereas perception is a change of quality in which there is only "maintenance" (*σωτερία*, 417b 3) or "development" (*ἐπίδοσις*, 417b 7) of a quality without the loss of an opposite quality (417b 3-7, 15-17). The difference seems to be only that water which becomes hot must lose the quality cold, but a sense organ becomes hot without losing the quality cold. This difference is obscure, because it is a logical truth that anything which becomes hot (that is, hotter than it was) must have ceased to be cold (that is, colder than it now is). But in any case Aristotle seems to emphasize only that no quality is *lost* in perception; he does not deny that a quality is *gained*. Therefore he has not shown how the gaining of heat by the sense organ differs from the gaining of heat by water.

[13] It is the becoming *x* in a *sense organ* which is being spoken of when this description of sense as receiving forms without matter is first introduced (424a 18). Sense is not a magnitude, *οὐδ' ἡ αἴσθησις μέγεθός ἐστιν* (424a 28), but it is a ratio or power of a magnitude, *ἀλλὰ λόγος τις καὶ δύναμις ἐκείνου* (424a 29). This "power" or "ratio" is said to be destroyed if it moves too far toward one extreme or another in its range of sensible qualities (424a 28-32 as glossed by 426a 31 – 426b 3). Thus sense here seems to be simply the power of a sense organ to change within a range of sensible qualities, e.g. temperatures or colors.

Thus Aristotle fails in his attempt to explain perception in terms of proposition *B*. While proposition *A* may be true in some cases, such as perceptions of hot and cold, it has no significance in a general theory of perception.

Aristotle should have asserted merely the following proposition, which I will label *C: an object which is perceived produces some sort of change in a sense organ.* While in our time this proposition seems obviously true, it represents a solution to the principal problem which Aristotle poses for himself in *DA* II, and it is a considerable advance over Empedocles' theory of sense perception as Aristotle conceives it. According to Aristotle, Empedocles held that what has a soul knows other things because the elements of those things are present in the knower himself as elements of his own soul (404b 9-15; 409b 23-26). The implication is that what has a soul knows other things by knowing the elements of those things in its own soul. Thus it would not really know other things *as other things* at all, but would only know elements of itself. In beginning the development of his own theory in *DA* II, 5, Aristotle asks why someone like Empedocles does not hold that we "perceive the senses themselves as well as the external objects of sense" (417a 3-4). The problem is to explain how we get knowledge of other things *as other things*. Aristotle puts the same point in another way by asking why someone like Empedocles does not hold that those having souls perceive "without [the stimulation of] external objects . . . seeing that they contain in themselves fire, earth, and all the other elements, which are the direct or indirect objects of sense" (417a 4-6).[14] Thus, Aristotle concludes that "what is sensitive is so only potentially, not actually" (417a 7) and that perception depends "on a process of movement or affection" (416b 34) by external objects. The problem is to explain how the individual having senses is brought from potentially perceiving external objects to actually perceiving external objects. The answer is that external objects affect the sense organs through various media, and Aristotle devotes the bulk of *DA* II, 7-11, to showing how the various sensible qualities of external objects affect the various sense organs. These

[14] I have bracketed the modern term "stimulation," used by the Oxford translator. No such word appears in the Greek.

specific formulations of the general proposition *C* represent a considerable advance over Empedocles' view and a solution to the problem of sense perception as Aristotle conceived it.

The mistake is to think that the conclusion explains more than it does—that is, to move from the conclusion *C*, that perception depends on the effect of objects on sense organs, to *B*, that in some vague way the effect on the sense organs is identical with perception and therefore that the organ becoming, for example, hot can explain the perception of heat. The mistake is to think that we can explain perception not only in the sense of specifying physiological phenomena upon which it depends, but in the sense that we can somehow make more clear what perception is and how it is that an animal has the power of discerning (424a 3-7). The attempt to provide such an explanation leads to the concepts of sense as a "mean," as "receiving forms without matter," and as becoming "like" things perceived, concepts which draw their meaning from various lines of thought about perception, and which therefore seem to have some explanatory value with respect to perception, but which in fact have none.

II

Not all of Aristotle's works reflect an analysis of perceiving *x* as a kind of becoming *x* in a sense organ. Thus in the *De Sensu* Aristotle says that perception "is generated in the soul through the medium of the body" (διὰ σώματος γίνεται τῇ ψυχῇ, 436b 7). He here suggests a distinction between an effect on the body, which is only the medium of perception, and an effect on the soul, which is perception itself. Similar statements are found in *De Memoria et Reminiscentia*, 450a 27-29, and *De Somniis*, 454a 7-11.

Hicks takes such passages as expressing a "neat summary" of Aristotle's account of perception in the *De Anima*.[15] However, they do not agree with the *De Anima* as I have interpreted it, and they probably belong to an earlier stage in Aristotle's thinking about soul and body. François Nuyens establishes three such

[15] *Op. cit.*, p. 483.

stages: (i) a Platonist view, found in Aristotle's early dialogues, in which the soul and body are opposed "*comme deux forces ennemies*"; (ii) a view found in most of the works on biological and moral subjects in which the soul and body are still thought of as independent substances, but now they "*collaborent à une œuvre commune et . . . cette collaboration est 'naturelle'* " (in this state, the power of the soul is thought to be exercized through a particular part of the body, usually the heart);[16] (iii) the view expressed in the *De Anima* in which the soul and body are no longer thought of as distinct substances, but the form and matter, respectively, of one substance.[17] Aristotle does use the language of soul "and" body in the *De Anima*, but he says that soul and body are one in the same sense in which the shape of wax (form) and the wax itself (matter) are one (412b 6-9). The unity of form and matter is for Aristotle the paradigm case of unity (*loc. cit.*). Thus one cannot construe the *De Anima* as saying that the effect of an external object on a part of the body, the sense organ, is distinct from perception itself, which is an effect on the soul. In the *De Anima* perception is presented as an event in a sense organ.

III

It remains to comment briefly on Aristotle's contrast between perception and the activity of *nous*, or intellect. By analogy with sense, which is said to receive "sensible forms," intellect is said to receive "forms," simply (429a 13-18). But while the activity of sense is bodily, as we have seen, the activity of intellect is said to be nonbodily (429a 24-26). Why this difference?[18]

[16] *L'évolution de la psychologie d'Aristote* (Louvain, 1948), p. 57.

[17] See *op. cit.*, pp. 57-58, for an outline of this view, and chs. ii, iv, and vi for detailed argument. It must be noted that Nuyens himself places both the *De Sensu* and the *De Memoria et Reminiscentia* in Aristotle's final stage (*op. cit.*, pp. 251-252). However, as Ross argues, texts like those cited, 436b 7 and 450a 27-29, show that Nuyens should have assigned both of them to Aristotle's second stage (W. D. Ross, *Aristotle Parva Naturalia*, Oxford, 1955, pp. 14-18).

[18] If intellect is nonbodily, it would seem that intellect could not be a faculty of the soul, since the soul is the form of the body. Aristotle suggests this as a possibility in *DA* I, 4, 408b 18-20. The relation between intellect and soul is not made clear in *DA* III, 4-5. On this subject see Nuyens, *op. cit.*, ch. vii.

Aristotle's arguments on this point in *DA* III, 4, are obscure and I shall not analyze them in detail. But I think that what lies behind these arguments is that Aristotle could conceive of, for example, perceiving red as a kind of becoming red, and therefore as a kind of bodily activity, but he could not conceive of the activity of intellect on the same model. For one thing, intellect apprehends not merely a set of sensible qualities within a given range, like the senses, but apprehends every sort of quality in every sort of range: "everything is a possible object of thought" (429a 18). Second, the forms which intellect receives are the "essences" of things (429b 10). The meaning of "essence" cannot be briefly stated, but understanding essences would at least involve a distinction of some kind between essential and nonessential properties. Aristotle could not conceive of an activity with these two characteristics as occurring in a bodily organ, and therefore he held that *nous* is nonbodily.

When one realizes that the perception of *x* cannot be explained simply as the becoming *x* of a sense organ, then the distinction between sense as bodily and intellect as nonbodily no longer seems plausible. Neither sense nor intellect is bodily in the sense that perception or thinking can be understood simply as a part of the body becoming *x;* yet both perception and thinking may be bodily in the sense that one can *correlate* specific kinds of perception or thinking with specific events in the sense organs or in the brain.

Could one refute Aristotle's claim that intellect is nonbodily simply by showing that every time anyone performs a mathematical calculation or makes a metaphysical argument, a certain characteristic change occurs in his brain? No. Aristotle's claim that thinking is impossible without sensory images (432a 3-8) here plays a crucial role. He could say that the recorded brain activity is that of the sensory faculty of imagination, and is thus only *indirectly* connected with the activity of intellect.[19] Indeed, on the supposition that the intellect itself has no bodily organ, a constant correlation of thoughts with brain states would serve as proof that thinking is impossible without the exercise of some sensory

[19] Aristotle held that the central organ of sense was the heart, not the brain (469a 10-23), but *mutatis mutandis* the argument is the same.

faculty, since it is thought that only a sensory faculty has a bodily organ. This sensory faculty is taken to be imagination. Thus we would have a proof of the claim that the exercise of the intellect is impossible without the exercise of the imagination. It then becomes difficult to see what factual observation could refute either Aristotle's doctrine that intellect has no bodily organ, or his doctrine that intellect is not exercised without imagination, when these two doctrines are held jointly. The only refutation which can be offered is to show that there is no a priori reason why specific acts of thinking cannot be correlated with specific events in the brain, just as specific perceptions can be correlated with specific events in the sense organs. One is then prepared to allow that the brain is the organ of intellect, and is not inclined to interpose a different faculty, the imagination, as the link between exercise of the intellect and changes in the brain.

THOMAS J. SLAKEY

St. John's College, Annapolis

DYNAMIC SIMPLICITY

I

THE important role played by the principle of simplicity as an adjudicator between rival scientific hypotheses has been widely acknowledged. But when philosophers have come to account for the operation of the principle, or for its logical basis, they seem to have encountered great difficulties. These difficulties have defeated all attempts to give a comprehensive and accurate description of the way in which the principle of simplicity has shaped the course of science.

On two important scores at least, nearly all explications of "simplicity" as a decisive factor in the ultimate acceptance of theories fall short. I shall discuss these shortcomings in the next two sections. Subsequently I propose to distinguish between the concepts of static and of dynamic simplicity, and to point out that philosophical attention has in general been focused on the former. This, I shall argue, has been the source of our failure to appreciate the true character of a principle which has been universally acclaimed as of fundamental importance to our understanding of the growth mechanism of the evolving structure of science.

At this point I shall give only a very brief indication of the nature of these two concepts. When simplicity is judged on the basis of a momentary state of affairs, we are referring to static simplicity. We are then comparing the simplicity of hypotheses as they are in the context of a given body of evidence and within the framework of current theory. Dynamic simplicity, on the other hand, is not associated with a given situation but with a continuous process. When the fortunes of two hypotheses have been followed over a period and it has been observed how, relative to an increasing body of evidence and changing set of theories, one hypothesis persistently becomes simpler to maintain than its rival, then we are referring to the concept of dynamic simplicity.

In the history of science the dramatic role of arbitrating between conflicting hypotheses has invariably been played by dynamic simplicity. Once this is realized, we have made a big step forward toward obtaining an authentic picture of the way the principle of simplicity has operated throughout the history of science.

II

The first deficiency to be found in all the explications of the notion of simplicity—one which becomes apparent on even the most cursory survey of the existing literature—is incompleteness. None of the attempts made so far has succeeded in giving an analysis in suitable terms so as to provide the principle of simplicity with a logical basis in all the cases where it is intuitively felt that the principle applies. In fact, they usually provide a very narrow basis relevant only to a few selected examples. At best most of these attempts may be regarded as hopeful suggestions for a program.

To illustrate the point, I shall examine in some detail K. R. Popper's celebrated analysis of "simplicity." The reasons for choosing this particular analysis are twofold. First, Popper's views on simplicity are important and influential, and have been approved[1] or even adopted[2] by other distinguished philosophers. Second, whereas most philosophers adopt toward the subject an attitude that is tentative in varying degrees, and they themselves acknowledge the incompleteness of their analysis, Popper categorically declares that through his explication "the epistemological questions which arise in connection with the concept of simplicity can all be answered."[3]

Popper's suggestion forms an integral part of his well-known

[1] E.g., J. G. Kemeny, "The Use of Simplicity in Induction," *Philosophical Review*, LXII (1953), 404.

[2] E.g., William Kneale or F. S. Barker. Kneale in his well-known *Probability and Induction* (Oxford, 1949), pp. 229-230, adopts Popper's views *in toto*. Barker in his *Induction and Hypothesis* (Ithaca, 1957), explicates simplicity in his own way but the essence of his views, that the simpler hypothesis always runs the greater risk of being contradicted by the evidence and that it therefore "says more" (pp. 181-182), is the same as Popper's.

[3] K. R. Popper, *The Logic of Scientific Discovery* (London, 1959), p. 140.

views on the nature of scientific theory construction. According to these views such construction should be governed by the central aim of producing theories as vulnerable to falsification as possible. The more falsifiable a theory, the more empirical facts it forbids, the more "it says" and therefore the more it is to be preferred. The principle of simplicity is merely a corollary to this more fundamental principle of always striving to produce theories with the highest degree of falsifiability. For Popper claims that in every case of conflicting hypotheses, that hypothesis which would intuitively be regarded as the simpler can also be shown to be the more falsifiable. Therefore:

> Simple statements, if knowledge is our object, are to be prized higher than less simple ones *because they tell us more; because their empirical content is greater and because they are better testable.*[4]

Popper does not advance a general proof for his thesis; he only produces a few examples in which greater simplicity and greater falsifiability do seem to go hand in hand. One of his examples concerns the comparison of the complexities of different curves. Such comparison is relevant to situations like the one with which Kepler was confronted. Given the positions of a certain planet at a number of different times, the question is which orbit—compatible with the recorded observations—is to be assigned to the planet. More generally, the relative simplicity of curves is of interest in those cases where we are given a number of readings, obtained by varying a physical parameter p with another parameter q. After each reading has been represented by a dot on the graph paper, it is the relative simplicity of the curves passing through these points that determines which one is chosen to exhibit the supposed relationship between p and q.

It is commonly assumed that a straight line, for example, is a simpler kind of curve than a circle, while the circle itself is simpler than, say, the ellipse. On the whole, a curve is regarded as simpler the fewer the parameters in the general equation representing it.

Thus, whenever it is compatible with the given data, a straight

[4] *Op. cit.*, p. 142.

line is to be preferred to a circle on the grounds of simplicity. The latter again is to be preferred to an ellipse, and so on. Now what Popper draws attention to is that a hypothesis involving a straight line is also easier to falsify than the one involving a circle. The hypothesis of a circular orbit or a circular graph is in turn more falsifiable than the hypothesis of an elliptic orbit or graph. And in general a hypothesis involving a curve determined by fewer parameters is always more readily refuted than the one with more parameters. This follows from the fact that observations specifying three points on the curve in question may already rule out the possibility of a straight line but not of a circle. A minimum of four points is required to refute a hypothesis involving a circle. For, given any three points, it is always possible to draw a circle through them. Again, four points given on a plane may rule out a circle but a minimum of five points on a plane is required to eliminate the possibility of a parabola. And in general $n + 1$ points must be given in order to contradict a hypothesis involving a curve with n parameters. Thus, concludes Kneale approvingly,

> the policy of assuming always the simplest hypothesis which accords with the known facts is that which will enable us to get rid of false hypotheses most quickly.[5]

Now the first thing that will strike us upon a somewhat closer examination of the matter is that Popper has not gone very far with the task of ordering curves according to their degrees of complexity. All he has done in fact was to place the straight line, the circle, the parabola, and the general conic into two-, three-, four-, and five-dimensional classes respectively.[6] Admittedly, according to Popper's definition of "the dimension of a set of curves," the parabola, for example, is to be placed into the four-dimensional class. The general equation of the parabola is

$$(ax + by)^2 + cx + dy = 1 \qquad \text{(i)}$$

The four parameters, a, b, c, and d, have to be specified before

[5] *Op. cit.*, p. 230.
[6] *Op. cit.*, pp. 130-133.

the parabola is uniquely determined.[7] It is therefore true that usually[8] at least five singular statements are necessary, corresponding to five points on the graph, in order to refute the hypothesis that the curve in question is a parabola. But the same is true, for example, about the curve represented by the polynomial of the third degree:

$$y = a + bx + cx^2 + dx^3 \qquad \text{(ii)}$$

or about the log-curve, which is most generally represented by

$$y = a \log (x + by + c) + bx + d \qquad \text{(iii)}$$

Given four points on a plane, it may be possible to draw through them a curve represented by (i), (ii), or (iii). Given therefore four points through which any of these types of curves may be drawn, which one shall we choose? In practice we shall probably choose (i), intuitively regarding it as the simplest,[9] but there is nothing in Popper's falsifiability criterion to make us prefer (i) to (ii) and (iii). The inadequacy of the criterion is more acutely brought home to us when we remember that in fact there is an infinite number of sets of curves, corresponding to the four-dimensional class or, for that matter, to any particular dimensional class. Before some means of distinguishing between curves belonging to the same dimensional class has been devised, one is inclined to regard the whole enterprise of ordering curves according to their complexity as something yet to be undertaken.

In the context of actual scientific technique it is possible to go further and dispute even the humble claim that by introducing the idea of the "highest degree of falsifiability" as an ultimate goal, an adequate reason for preferring curves with fewer parameters has at least been provided. Let us suppose that the variation

[7] Strictly speaking, four points determine a pair of parabolae; a fifth point is needed to decide between the two.

[8] But not always. When the quadrilateral formed by these four points is re-entrant, no real parabola can be drawn through them. (See Charles Smith, "Elementary Treatise on Conic Sections," London, 1889, p. 234.) Popper is inaccurate in implying that a parabola can be drawn through any four points in a plane.

[9] This could perhaps be justified informally by saying that although (i) has more terms than (ii) it is an equation of the second degree only and that (iii) is to be regarded as more complex since it is a transcendental equation.

of a physical parameter p with another, q, has been observed and that a dozen readings have been taken and represented as dots on a graph. Further suppose that no three of these dots lie on a straight line, but that a straight line could be drawn in such a manner that none of the points would be "too far off" it. The practical scientist, as we know, will draw the straight line and maintain that p varies linearly with q.

Imagine, however, that we took a very stringent view of the way in which our theories should represent experimental data and that we were not prepared to assume that the relationship between p and q was a linear one so long as there was any deviation from a straight line by any of the dots representing a given instance in the co-variation of p and q. In this case we shall prefer some twelve-parametric curve which, though rather complex, could fit the recorded readings.

The adoption of such an uncompromising experimentalism, it might be claimed, would result in making our hypotheses much more highly falsifiable. For as soon as the thirteenth reading was given and it was found slightly off the complex curve, the hypothesis that it represented the relationship between the two physical variables would be refuted. But not so with the practical scientist. The straight line on his graph constitutes a demarcation line of the margin of permissible variance with the given data. The possibility of further dots off his line does not necessarily present a threat to his hypothesis as long as their deviation does not exceed this margin.

This example would tend to show that in practice the tendency to construct theories with the highest degree of falsifiability and the tendency to make them as simple as possible may run counter to one another. The former would cancel the license to deviate in our representations from the recorded data; the latter would extend it indefinitely.

One could, however, resist this argument by maintaining that falsifiability is an objective predicate of hypotheses independent of the standards set by any individual demarkation. If the objection is sustained, then I have failed in this instance to show that simplicity and falsifiability may sometimes become contrary attributes. But no defense could go any further and even maintain

that they are parallel attributes. For in this last case, the case in which the number of points given exceeds the number of parameters in the equations of the simpler curve, falsifiability certainly no longer co-varies with simplicity. A thirteenth point is surely no less a potential falsifier of the complex-curve hypothesis than it is of the straight-line hypothesis.

In general, outside the realm of curves, there are many examples in which falsifiability does not go hand in hand with simplicity. There are also examples where the degree of falsifiability increases as the degree of simplicity decreases.

Not all hypotheses, for instance, concern theories, laws, or relationships. Some postulate concrete states of affairs. Now whenever hypotheses differ from one another by the number of physical entities whose existence they postulate, the simpler hypothesis is not falsifiable to a higher degree but, on the contrary, to a lesser degree. For the more elements a hypothesis stipulates, the more complex it will usually be considered. At the same time, it is easier to falsify such a hypothesis when it is sufficient to obtain evidence that any one of the many elements required by it is missing, and the complex hypothesis is thereby refuted.

A fictitious historical example can be provided if we imagine that the following took place in 1846 when Leverrier and Adams tried to account accurately for the observed perturbations in the movements of the planet Uranus. Suppose they had arrived at two unique solutions; that is, they had obtained a second solution in addition to the one which led to the discovery of Neptune. Suppose that the first solution stipulated one planet at a given position, and the second stipulated a dozen planets of larger magnitudes, but so situated as partly to cancel each other's effect and still give rise to the same perturbation as the single planet of the first solution.

It is, of course, an entirely irrelevant question whether it is mathematically possible to find a unique configuration of a dozen planets to replace the effect of Neptune. The point is that *if* such a unique alternative hypothesis were available, it is quite clear that we should intuitively reject it in favor of the Neptune hypothesis. But here again the more complex hypothesis is the more falsifiable one. We may choose any of the twelve specified

regions in the sky (and some regions are more easily searched than others), explore it, and, if no planet is found in it, declare the hypothesis refuted. Besides, as these twelve hypothetical planets are of larger magnitudes than the single one, the failure to sight them with weaker telescopes will also count as a falsification.

This last type of situation, where there is a choice of assuming one out of a number of concrete states of affairs which vary in their complexity, is particularly common in historical research and crime detection. Historians and detectives do and should use the principle of simplicity in their work, and they may expect philosophers to cater for them, too, when explicating the notion of simplicity.

III

So far, the point made was that whenever it has been attempted to define precisely the term simplicity and provide the principle of simplicity with a logical basis, the basis always turned out to be too narrow and relevant only to a limited range of cases. The second point I am about to make is that even the cases which are within this limited range are not real ones. They represent neither examples taken from the actual history of science nor examples that are ever likely to occur and in which the principle of simplicity would be called upon to adjudicate between theories held by different scientists.

This failure is inevitable, and one cannot place the blame on those philosophers and mathematicians who have attempted to give the static concept of simplicity a rigorous treatment and did not succeed in rendering their explicated concept applicable to any historical example in which the principle of simplicity did play or could have played a deciding role. The reason for this failure may briefly be given as follows. To compare rigorously the complexities of two theories it is first necessary to know what these theories are all about. It is certainly not enough to understand some features of two theories and compare them with respect to these while ignoring all other significant features. No amount of mathematical ingenuity can provide an apparatus

to compare the complexities of hypotheses of unknown or of only vaguely known natures.

The important point, however, is that in all the noted historical cases where there has been intense rivalry between two theories, the full nature and implications of these theories were not realized at the time. Furthermore, the disputants were aware of their inadequate appreciation of the implications of the theories they themselves were advocating. Therefore, the various principles of simplicity put forward by different philosophers as means by which these disputes could have been settled would in fact have been of no avail.

Let us consider, for instance, the famous example of the prolonged conflict between the two planetary systems, the Ptolemaic and the Copernican. It is often said that the Copernican system is the simpler since it employs fewer epicycles in its account of the planetary motions. For our purposes here we may ignore the fact to which some historians of science have recently drawn attention, namely, that the number of epicycles featured in the Copernican system is actually much larger, Copernicus' own statement to the contrary notwithstanding. Let us ask ourselves: if the number of epicycles employed by Copernicus had indeed been considerably smaller than the number used by Ptolemy, would the relative simplicity of the Copernican hypothesis have been thereby guaranteed?

The answer, clearly, must be: No. For the difference between the two planetary theories is vastly greater than the mere fact that they employ a different number of epicycles. In order to be able to estimate correctly the comparative simplicity of the two theories, one must take into account their immensely numerous implications. Our verdict as to which world scheme is ultimately simpler will depend on what sort of knowledge of the laws of dynamics we possess.

It will suffice if we consider just one of the many reasons why the Copernican system was rejected for a long while. If the earth moved, it was argued, objects thrown vertically upward would fall westward as the earth moved away from beneath them. The fact that this does not happen amounts to an experimental refutation of the moving earth hypothesis. Clearly, as long as this

objection remains unanswered, no appeal to the simplicity of the Copernican system can be made. The principle of simplicity can only decide between two hypotheses when they account equally well for observed phenomena.

But the objection, as we know, can be met. It can be met by suggesting that the earth's rotation need not imply the westward fall of projectiles since these share the ground's eastward movement before projection and continue to do so even after they leave the ground. In other words, we propose the adoption of the law of inertia. This eliminates our immediate difficulty but introduces numerous other difficulties. After all, it does seem evident that a moving body will stop unless continually acted upon by a force. Aristotelian mechanics, which denies the law of inertia, seems to be borne out far better by everyday happenings than the Galilean-Newtonian mechanics. If we insist upon the truth of the law of inertia we shall have to introduce, in the most simple and straightforward cases, retarding forces such as air resistance or friction, so as to explain why the law of inertia seems to be persistently violated.

Of course, we now know how the introduction of the law of inertia and the other basic laws of modern mechanics have brought about undreamed-of simplification, unified the whole of celestial and terrestial mechanics, and connected many previously unrelated phenomena. But all this was not and could not have been foreseen in the sixteenth and early seventeenth centuries.

The point I am trying to make is that at no stage could one have determined the relative simplicity of the two planetary systems by merely counting the number of epicycles respectively employed by them. This would seem an innocuous enough claim to make, yet it is not always conceded. A case in point is Margenau, who says:

> As an illustration of the principle of simplicity, the heliocentric discovery has a peculiar appeal because it allows simplicity to be arithmetized, it involves the reduction in the number of epicycles from 83 to 17.[10]

[10] H. Margenau, *The Nature of Physical Reality* (New York, 1950), p. 97.

A more striking example of oversimplification is provided by Kemeny in the paper mentioned above. Toward the end of the paper he states:

> Had Copernicus been familiar with the results of this paper he could have argued as follows: the planets move in a plane closed curve around the sun. We must consider families of such curves and order them according to their simplicity.[11]

He then goes on to show how, by the mathematical method he had constructed, ellipses are simpler geometrical curves than the combination of epicycles, and therefore:

> In this one step (of assigning one ellipse rather than a set of circles to the orbits of planets) Copernicus could have anticipated Kepler's main result *from purely methodological considerations*.[12]

This example is not basically different from the one dealt with before, except that the gap separating the Copernican and Keplerian systems is perhaps somewhat less wide than the one that exists between the Ptolemaic and Copernican systems. But it still goes without saying that the difference between the two solar systems is not merely confined to the differences in the number of parameters determining the shapes of the respective orbits. In fact, most probably Copernicus, like everybody else, would intuitively—without being aquainted with Kemeny's more rigorous method of comparison—have regarded the ellipse as a simpler type of geometrical figure than the combination of epicycles. But the fact that an ellipse is a simpler geometrical curve does not necessarily mean that it is also a simpler planetary orbit. The relative simplicity of planetary orbits is determined by many more factors than the purely geometrical ones.

On adopting an elliptic orbit instead of a circular orbit, Kepler assigned "unnatural" or "violent" motion to the planets which hitherto had been thought to move only along "perfect" circular paths. Such a change had far-reaching implications affecting the very foundations of mechanics. The ultimate consequences were bound up with equating the laws of celestial and terrestial mechanics; appreciating that the law of inertia applies to both;

[11] *Op. cit.*, p. 404.
[12] *Loc. cit.*

recognizing that curvilinear motion, of no matter what form—circular, elliptic, or anything but straight—is a constrained motion; introducing universal gravitation; and so on. Of course, here again, the precise nature of the repercussions of the transition from one system to the other was not foreseen either by Copernicus or by Kepler though both had some inkling of the complexities involved in abandoning circular orbits which were axiomatic in astronomy for nearly two thousand years. At any rate, it was appreciated by them that the matter was not a problem confined to pure geometry alone. Thus it is obvious that the mathematical apparatus devised by Kemeny for the comparison of the complexity of curves would have been of no help to Copernicus.

The point illustrated by these last two historical examples is of general relevance. Examine closely any of the known conflicts in the history of science and you will see that while the controversy was raging only a small fraction of the implications of the rival theories were understood by members of either side of the dispute. Rigorous comparison at the time was not possible. Today, of course, we may, for practical purposes, regard most of classical physics and chemistry as forming a closed system. This means that the implications of various classical theories may be regarded as known. We can, therefore, take Ptolemaic astronomy, or the phlogiston theory of combustion, or the caloric theory of heat, or the corpuscular (Newtonian) theory of light, and demonstrate in the context of our present system of knowledge how immensely more complicated it would be to hold these theories than their rivals. But today, of course, there is no longer any controversy to settle. Do we then conclude that while the battle was on, the principle of simplicity could be of no help, and now when it could be of help its services are no longer needed? No, the principle of simplicity has played a role in establishing each one of these theories, but this role has been different from what it is often thought to be.

IV

The concept of simplicity to which I shall now be referring is a dynamic attribute of theories. The principle of simplicity

which is genuinely operative in settling scientific controversy does not apply to a given situation but to a continuous trend. This principle does not act instantaneously but in the course of a process in which the simplicity of the correct theory asserts itself to an ever increasing degree. The logical basis of this principle is at the very foundation of our whole concept of empirical truth.

A brief example may be of help here. Tom and Dick, who have just moved into the neighborhood, are known to be related to one another but the nature of their relationship is not known with certainty. Some people entertain H_1, namely that Tom is the father of Dick, on the basis of vague hearsay. Others, also relying upon uncertain sources of information, hold H_2, that Tom is the brother of Dick. Let us assume that H_2 is the true one but that for the moment, apart from these rumors, there is no further evidence supporting either of them. There is no good reason to prefer one assumption to its alternative. H_1 is just as simple or complicated as H_2.

Gradually, however, our store of information relevant to the truth of either H_1 or H_2 increases. Eventually we may have the opportunity to see these people and we may observe that they look the same age. This fits well with H_2, the brother hypothesis, but not with H_1, the father-son hypothesis.

It is, however, not the practice either in science or in everyday life to reject a hypothesis just because one adverse point comes to light. What is usually done is to introduce some slight modification in the original formulation or to stipulate an auxiliary hypothesis so as to ward off the ill effects of the refuting instance.

The supporters of H_1 will accordingly perhaps suggest that Tom, who is really the father of Dick, is a very vain man who by various means (massaging, dieting, and dyeing his hair) has succeeded in reducing his apparent age well below his real age. This suggestion, of course, makes H_1 slightly less straightforward than H_2. H_1 will become even more cumbersome when further special assumptions about the unusual type of vanity with which Tom is afflicted will have to be introduced when, after having had more opportunities to observe him, we notice for instance how invariably slovenly he dresses.

The end of this story seems to be inevitable. It seems perfectly

obvious to us that since Tom is in fact the brother of Dick, more and more evidence—the manner of their behavior toward each other, the way in which they refer to each other, statements made by their friends—all ostensibly supporting this fact is bound to come to our notice in time. Naturally, just as before, each piece of evidence can be "explained away." But this means the continual addition of independent auxiliary hypotheses. In contrast, H_2 will unify all those phenomena that need independent explanations under H_1. Eventually a situation will be reached where H_2 is so overwhelmingly simpler that even its staunchest supporters will abandon H_1 as the false theory and accept H_2.

This in essence is the process whereby all hypotheses previously current but now abandoned as false have been overthrown. This is, for example, the way in which we have come to believe in the falsity of the proposition that the earth is flat. Not that it was unreasonable to hold the flat earth hypothesis in ancient times, but it has become progressively more and more so with the increase in navigation and the accumulation of astronomical observations. By "unreasonable" one means, of course, that it requires a fantastic number of special assumptions in contrast to the reasonable hypothesis which unifies, by the mere assertion that the earth is round, so many otherwise unconnected phenomena. The phenomenon that the lunar eclipse always has a circular edge does not by itself conclusively show the roundness of the earth. After all, it is possible that it is not the shadow of the earth which causes the eclipse. The phenomenon that the hull of a receding ship disappears before its mast may also be explained by some optical hypothesis. But both phenomena, and others whose number kept increasing all the time, are much more simply explained once we agree that the earth is round. That is why at one stage or another everybody came out in favor of the round earth hypothesis and in doing so thought he had come to "recognize the truth." This is a rather obvious illustration of the operation of the principle of the persistently increasing relative simplicity of the correct theory or, in brief, the principle of dynamic simplicity.

I have not accounted for static simplicity. I have not explained how at a given instance on grounds of simplicity some hypothesis

is chosen in preference to others. Mine was an account of the way conflicts between hypotheses actually held by opposing camps become settled, but not of the way these were originally selected out of all the possible hypotheses. It was an account of the function of simplicity in the history of science, a description of the process of continuous simplification in the course of which the truth of one of the hypotheses manifests itself to an ever increasing degree. But it is this dynamic simplicity which has been the source of all the puzzled speculation and the cause of all the fascinated concern with simplicity in science. People were—and still are—impressed greatly by the continuous simplification that goes on in science and by the fact that in all the conflicts the simpler theory seems to come out victorious. Many great scientists and philosophers have taken this as a sign of the simplicity of nature itself and concluded that the universe is ultimately governed by simple laws. The view suggested here is that what we have is not a reflection of the nature of the universe but of the nature of our notion of truth in general.

It is always easier in the long run to defend the truth. The defenders of untruth are bound to become entangled in an ever growing mesh of explanations they have to devise in order to keep their theory afloat. In comparison, the champions of truth find themselves in an increasingly simpler situation. There is wisdom in the age-old saying, "The seal of truth is simplicity." It needs, however, to be added that often this seal is concealed from us. But with the passage of time it invariably emerges and its features become more and more conspicuous with the accumulation of relevant knowledge.

G. SCHLESINGER

Australian National University

PROPOSITIONS

ARE there such things as propositions? Quine says there are not.[1] He says that they are metaphysical entities like the entity "meaning." The notions of "meaning the same" and "saying the same thing" and of synonymy should be dropped in favor of the notion of significance. What is significant is a "sound sequence" and it is significant when the people hearing it show no "reactions suggesting bizarreness of idiom." Ayer says that propositions are a class of would-be invented facts.[2] Geach says that it is doubtful whether there is such a concept as the concept of propositional identity.[3] If there is, it is confusing and obscure.

There are two sorts of reason for rejecting the notion of proposition. One is that the notion is superfluous. Anything that can be said by talking about propositions can be said better by talking about something else—sentences, say, or words, or sounds. Very often the superfluity is attributed to the alleged metaphysical character of the concept of proposition. The concept is said to be metaphysical and *therefore* superfluous. The other sort of reason for rejecting the idea of proposition is that it is obscure. It is obscure because it is difficult to give a satisfactory account of what a proposition is, or to find an uncontroversial definition of the term, or to state in what the identity or nonidentity of a pair of propositions consists. It is difficult to find any simple and universally applicable tests for the identity and nonidentity of propositions.

I shall examine the two sorts of objection in turn.

The chief motive for talking about propositions is that there is a difference between the identity of sentences and the identity of "the thing that is said." The identity of sentences lies in such things as the identity of words and the identity of word order, which together constitute a sufficient condition of identity. Of

[1] *From a Logical Point of View* (Cambridge, Mass., 1953).

[2] *The Foundations of Empirical Knowledge* (London, 1947), p. 96.

[3] "Entailment," *The Aristotelian Society*, Supplementary Volume, XXXIII (1959), 160.

course "identity of words and word-order" is not entirely clear: thus one sentence can be written or read out or even put into dumb show. But the puzzles that arise here are not particularly interesting ones.

The point of talking of the same sentence arises in such situations as trying to read bad handwriting, taking dictation (especially in a foreign language), and deciphering manuscripts and hieroglyphics.

The simple identity tests which are applied to sentences cannot rule out the possibility of ambiguity of reference or of meaning. Nor can they insure that there is one way and one way only of saying the same thing about the same object or person or state of affairs. Therefore it is possible (as is well known) that (a) two sentences may say the same thing about the same thing, and (b) one sentence may on different occasions say two things, or refer to two things, or both.

This may come about in a variety of ways. It may come about because the sentence or sentences contain personal pronouns, which of course can be used to refer to anyone at all. Or they may contain demonstratives such as "this," "that," "here," or "now," which may be used to refer to anything: state, time, or place. Or it may come about because there happen to be several ways of referring to one person (say) or several ways of saying the same thing about him. For instance: "Tom weighs the most" and "Tom is the fattest," or "A is bigger than B" and "B is smaller than A." Or again, it may come about simply because someone says something first in French and then in English.

There seems to me to be no point in saying that there are no such things as propositions unless this means that there is no such thing as the thing that is said, over and above the sentence saying it. But this is manifestly false. It might be argued that ambiguity of reference, or any other kind of obscurity, is not an essential feature of sentences and can always in principle be eliminated by, for example, pointing or more talk, and that therefore the need for the notion of proposition is illusory. It is the possibility of obscurity or ambiguity, however, and not the mere fact of its existence which creates the need for the notion

or, rather, which helps create the need. Similarly, it is not the mere fact that at least two languages exist, but the possibility that two might have existed even if it were supposed that only one did exist, that provides another reason for talking of propositions.

Sometimes it is said that propositions are metaphysical super-sentences or other objectionable and impossible entities. They are metaphysical entities rather in the way that Platonic forms and abstract ideas are metaphysical entities. Let us suppose that someone is asked, "What is the proposition which all these sentences, S_1, S_2 . . . S_n, express?" He might (a) repeat one of the sentences, (b) make up another sentence, S_{n+1}, (c) invent an ideal language and utter one of its sentences, or (d) refuse to answer, saying "metaphysical entity."

Any of these replies might be the wrong one. The first two answers might be taken to imply that a proposition is a sentence. The third answer might be taken to imply that propositions are some sort of forms. Then the notion of proposition will lead to vicious regresses and other troubles. ("What is it that both the sentences and the proposition say?") The fourth answer might be taken to imply that propositions are unknowable somethings lying behind the phenomena of sentences.

But then, any of these answers might after all be the right answer. Compare the request "Show me Othello." If someone in response points to an actor on a stage, he might get the reply, "That is only Garrick/Olivier/Robeson/Ivan Popov." If instead he says, "I cannot *show* you Othello," he might get the reply, "So Othello is a metaphysical entity." But on the other hand, these explanations of "Othello" may indeed help someone to grasp the idea of what it is to be a character in a play.

Those who think that by giving a peculiarly well-made sentence (perhaps in a special language) one actually produces the proposition, and those who think that to give such a sentence when asked "What do all these sentences express?" is an admission of failure, are making the same mistake. As the proposition is not a super-sentence, ordinary sentences do not resemble the proposition more or less closely; consequently, there are no sentences which because of their shape or form are better at expressing a

proposition than others—assuming, of course, that we are comparing only sentences which are all grammatically correct.

PROPOSITIONS AND STATEMEMTS

The term "proposition" is not usually taken to cover questions and commands as well as assertions. But the nature of the motive for introducing the idea in the first place makes this limitation pointless. It is possible for one question to be asked in different ways, or for one sentence on different occasions to ask different questions. It is possible to repeat a command, request, or order at greater length, or more succinctly, and also possible to issue the same command more clearly, repeat the very same request but in politer terms, and so on. The reason for limiting the term "proposition" in this way has nothing to do with the distinction between sentences and propositions, but is that questions and commands do not appear in truth tables. The same consideration is the reason why "proposition" does include that which is said or meant by (some) parts of sentences. It would be useful if the term "proposition" always meant "the thing that can be said or denied or asked or commanded by the use of the (complete) sentence(s) S_1 (S_2, S_3, . . .)," and the term "statement" always meant "the thing that can be said by S_1, S_2, . . ." plus "the thing that can be said by part of S_1 . . . ." But it is too late now. As it is, the term "statement" usually seems to mean "that which is expressed by a complete sentence in the indicative mood" and "proposition" the same, plus "that which is expressed by those parts of complex sentences which themselves can have a truth value."

Of course the way in which two sentences can be used to attribute the same property to the same object (say) is quite different from the way in which two sentences can be used to ask the same question about the same object. It is a different kind of sameness. To place too much weight on this, however, might beg the question as to whether or not there is only one way in which two sentences can make the same assertion: only one standard, test, or group of tests for statement identity.

In what follows I shall use the word "proposition" throughout.

Whether what is said applies to statements only, or to questions commands, and parts of statements also ought to be clear from the context each time.

WHAT THE PROPOSITION IS

The second reason for objecting to the notion of proposition was that no simple satisfactory criterion of identity for propositions can be found. It is certainly true that many possible criteria have been suggested and that most are unsatisfactory for one reason or another. The trouble is that there is some point to even the worst of them, while anyone with ingenuity can think up examples to which even the best will not apply.

It is not possible to give a neatly subdivided list of all the criteria which have been suggested, but, roughly speaking, they are of three main types: (1) those which concern utterances and sentences and the words in them; (2) those which concern sentences and their meanings; and (3) those which treat truth values as essential conditions of identity.

(1a) Some philosophers have said that a proposition is an utterance, that is to say, some one historic event which occurs at some one time and which consists of someone's uttering certain noises.

(1b) A proposition, it has been said, is a sentence.

(2) Moore said that a proposition is that which is apprehended when a sentence is heard and understood. That which is apprehended is the meaning of the sentence.[4] The concept of meaning, however, is no less difficult than that of proposition.

(2a) For example, it has been said that the meanings of words are the things for which the words stand or to which they refer, and the meanings of sentences are the facts which the sentences state or refer to. On this view of meaning, two sentences would express the same proposition if and only if they referred to the same object or stated the same fact.

(2b) Another definition of meaning is: the meaning of a sentence is its method of verification. On this view of meaning,

[4] G. E. Moore, "Propositions," in *Some Main Problems of Philosophy* (London, 1953), p. 57.

two sentences would express the same proposition if and only if the methods of verifying the one were identical with the methods of verifying the other.

(2c) An account of propositional identity which seems to fit here, although its author does not like words like "meaning," "proposition," and so on, is that mentioned by Quine.[5] He suggests that two sentences are synonymous when the situations in which they are uttered and the responses which they evoke have a certain degree of similarity.

(2d) "Meaning is use." This view of meaning opposes the meaning of sentences to the things to which they refer and to the facts which they state. According to this view of meaning, if a proposition is the meaning of a sentence, then two sentences express the same proposition if and only if the rules for the use of the one sentence are exactly the same in form and content as the rules for the use of the other.

(2e) Entailment: two sentences mean the same if they entail one another. If they entail one another, they express one proposition.

(3) It is often assumed that the identity of propositions depends on their truth values. This is in fact implied by each of (2b) and (2e); (2d) on the contrary implies that the truth values of one proposition can change; (2c) seems to me to be ambiguous on this point, and the remaining suggested criteria do not imply that the truth value of a proposition is either essential or inessential.

The defects of the tests set out under (1) are well known so I shall state them only very briefly. They are as follows.

Since an utterance is spoken rather than written but in any case cannot be repeated (any repetition constituting a new utterance) and since the word "utterance" suggests moans, groans, and cackles as well as words, it is not at all like the notion of the proposition as "that which is said." "That which is said" as against "that which is uttered" is essentially meaningful and repeatable.

Some of the objections to equating propositions and sentences

[5] *Op. cit.*

have already been set out above. Some further objections are these: Sentences are either French sentences or English sentences or Russian or Chinese, and so on. If a sentence is in Chinese, it is not the same sentence as a sentence in French, but they can both express the same proposition. "*S* is a sentence in French" makes sense; "*P* is a proposition in Russian" does not. Sentences can be grammatical or ungrammatical; they can be analyzed and parsed; when written, they frequently contain capital letters and punctuation marks; when spoken, they can emerge quickly or slowly, in a loud voice or a soft one, and they can be broken or interrupted by sobs and laughter. None of these things is true of propositions.

A sentence has a meaning. It is courting confusion to ask "What is the meaning of the thing he said?" (that is, of the proposition). To say that the noises he made had meaning is to say (usually) that they were words; to say that his words had meaning is to say (often) that he succeeded in forming some kind of sentence; to say that his sentence had meaning is to say that he managed to say something—for example, that what he said was not only grammatically correct, it was also not nonsensical. But to go on and ask "Did what he say have meaning?" does not now make sense since we have excluded the sense of "what was said" in which it can make sense (that is, "Were they words? Was it a sentence?" and so on). "Was his meaningful sentence significant?" can mean only "Was it significant in such-and-such a context?" (for example, significant as an indication of the speaker's mood).

Word-word equivalence is clearly not a necessary condition of the synonymy of sentences nor of the identity of propositions. It is not a sufficient condition, either, unless a great deal else is assumed. "*Tom ist hier*" and "Tom is here" express the same proposition only if we assume that "Tom" refers to the same person each time.

From the fact that the conditions of the identity of utterances or of sentences or of words are different from the conditions of identity of propositions, it does not follow that an examination of utterances, words, or sentences may not be a necessary part of the process of establishing the equivalence or nonequivalence of two propositions. The conditions of identity for pieces of wood

and ivory are not the same as the conditions of identity of the king, the pawns, and so on. But in order to move the king, one has to move a piece of ivory, and to show what the king is involves pointing to the piece of ivory. A proposition is a sentence in the same sense in which the king in chess is a piece of ivory.[6]

In many contexts, to establish that two sets of words are or are not identical is in fact sufficient to establish that two propositions are or are not one and the same proposition. For instance, if on page 24 we see the words "Sherlock Holmes spent Tuesday in the cellar" and on page 26 again the same words "Sherlock Holmes spent Tuesday in the cellar," we know that the very same proposition is asserted on pages 24 and 26 simply because the same words occur. The questions "Was it the same cellar? The same Tuesday? The same Sherlock Holmes?" do not arise.

I turn now to those conditions of propositional identity which concern sentences and their meaning. The first of these was (2a): two sentences express the same proposition if and only if they refer to the same thing, person, or state.

If someone taking a Gallup poll visited everyone living in a certain street and said to each of them the words, "Do you like gardening?" would he not have asked each of them the very same question? The question is about gardening; but it is also about the "you" concerned, and this reference changes each time the question is asked. Similarly, if the Gallup pollster ended each interview with the words, "If you do, so much the worse for you," has he not said the very same thing to each? Has he not made the very same statement to each? If it is accepted that he has, then even mutual entailment is not a necessary condition of identity for all types of proposition.

To restate (2b): two sentences express the same statement if anything which tends to show that the statement expressed by the first is true (or false) also and to the same extent tends to show that the statement expressed by the second is true (or false).

The first defect of this test is that it applies only to statements and not to propositions as a whole. This would not matter so much if it applied to all varieties of statement, but it does not

[6] This analogy is Miss G. E. M. Anscombe's.

even do this. Since it concerns evidence it cannot be of use in dealing with analytic statements. For the same reason, it is of no use in dealing with many pairs of sentences about pains, tingles, and the like. If the verification test is regarded not as a necessary condition but simply as a test or yardstick, it is still not very useful, for the reason that it is not right to assume that the notion of "same verification" is a simpler notion than that of "same statement." "The ex-factory hand has a moustache" and "The present Generalissimo has a moustache" are about the same person and say the same thing of him. But is it the case that anything which tends to prove the truth of one tends to show the truth of the other to the same degree? Is one not sometimes inclined to say that a piece of evidence E_1 is "really the same evidence" as E_2, simply because E_1 is related to p as E_2 is to q and one has already decided that p and q are identical?

As (2c) stated: there is, when two sentences are synonymous, a similarity of situations which evoke the two sentences, and a similarity of effects on the hearers.

The test mentioned by Quine is presumably to be understood as requiring a knowledge of the internal state of the speaker as part of the first situation. This Quine admits is a difficulty. A more serious difficulty is that there is no indication in the world as to what constitutes relevant similarity of situation or reaction. For instance, do the things said by other people in a discussion count as relevant when deciding that the evoking situation of S_1 is the same as the evoking situation of S_2 (where S_1 and S_2 are sentences)? Is a reply to count as an effect? If some evoking situations are discussions and some effects are replies, then it is arguing in a circle to describe meaning and synonymy in terms of similarity of evoking situations and effects.

The very same statement may be uttered or written in response to quite different situations. P may be first uttered in anger and in order to wound (say) and later written down calmly as part of a chapter in an autobiography. Smith might be delighted to hear q, but Jones might be thrown into despair. The delight and the despair are both effects. Unless an enormous amount was already known about the state of mind of Smith and Jones, these

effects would not be of the slightest use in determining whether S_1 and S_2 both expressed q.

According to (2d), the two sentences must be used in the same way.

When P. F. Strawson said that it is precisely because a sentence has clear, unambiguous meaning that it can be used to say different things on different occasions, he was not talking about anything like the particular reference made on a particular occasion by a sentence, but about a less specific feature of the sentence, namely, the way it is in general used.[7]

If the use of a sentence determines its meaning, then "so-and-so is happening now" always means the same: if the reference determines the meaning, "so-and-so is happening now" means something different every time it is uttered because the time referent "now" keeps changing.

"Two sentences make the same statement only when they mean the same" is clearly false if by "meaning" is meant "use." The uses of sentences containing proper names are not the same as the uses of sentences containing personal pronouns and demonstratives. But "Tom did it" and "That man did it" can express the same proposition. Again, the use of "I" is different from that of "he" and "you." The report of another person's intention to a third person is a different sort of thing from an expression of one's own intentions. Yet from the point of view of the third person, the hearer, the information might be the same.

Sentences with the same kind of use can make different statements on different occasions, and sentences with different kinds of use can sometimes make the same statement as each other. Sometimes it is precisely difference of meaning, in this sense of meaning, which is cited as a reason for saying that different statements have been made by two sentences. This is partly because "kind of use" covers such a variety of things. Thus perhaps past-tense expressions exemplify a different kind of use from present-tense expressions; but "He visited his friend in hospital yesterday" may say the same thing as "He is visiting his friend in hospital." A particular question exemplifies a

[7] P. F. Strawson, "On Referring," *Mind*, LIX (1950), 320 f.

different kind of use from a particular command and here indeed "kind of use" is a mark of identity or, rather, of nonidentity. "Must you go?" and "You must go!" are different because they exemplify different kinds of use.

In (2e) mutual entailment is mentioned. One objection to mutual entailment as a test of synonymy is that raised somewhere by Moore, namely, that sometimes a short simple sentence and a very long complicated sentence may express propositions which mutually entail each other. Moore said that the proposition is that which is apprehended when a sentence is understood: it is therefore natural that he should reject the idea that a sentence which was hard to understand could say the same thing as one which was easy to understand.

Another objection is that entailment is undoubtedly a more difficult notion than the notion of propositional identity, and indeed the natural tendency is to explain the first in terms of the second and not vice versa. This means that entailment is not a good test of identity. It may nevertheless, of course, be a necessary and sufficient condition of identity.

There do, however, seem to be pairs of sentences which on some occasions at least count as saying the same thing, yet the things they say do not entail each other. Thus one can say the same thing to different people, yet if what is said is about the listener, there is no mutual entailment. Second, some people say that if one says, for example, "Tom is slim" on two occasions at an interval of years, one will be saying the same thing about the same person; yet the passage of time makes what is said the second time into a falsehood. This does not seem a self-contradictory or incoherent view of propositional identity. There cannot, however, be mutual entailment between a truth and a falsehood.

If identity of meaning is identity of entailments, then meaning and truth are very closely connected.

Thus, identifying propositions in terms of entailment clearly connects with the question, can a proposition change its truth value?

A proposition cannot change its truth value if it is a question or a command or any other expression having no truth value.

Necessary propositions and self-contradictory ones surely cannot change their truth value, either.

Many things affect the truth values of propositions. Or to put it another way, a sentence might express a proposition which is true, or it might express one which is false, and this will depend on many things. It may depend on whether a proper name—for example, "Tom"—refers to this Tom or that. It may depend on whether a referring term such as "he" or "it" refers to this or that *he* or *it*. It may depend on to whom an utterance of the sentence containing the term "you" is addressed. It may depend on the speaker, if the sentence in question contains the self-referring terms "I," "now," or "here"; or it may depend on the place or time of utterance.

Sometimes "Tom" refers to Smith and sometimes to Jones, and few people think that "Tom is tall" expresses the same proposition when first Smith is meant, then Jones, although we can say that the same thing has been said about two men. "Today" sometimes refers to a Wednesday in June and sometimes to a Friday in April; and many people think that "Tom is at home today" always expresses the same proposition—given the same Tom—in spite of this fact about "today" and in spite of possible changes in truth value. The distinction between truth-value changes which involve identity changes for propositions and those which, supposedly, do not does not parallel the distinction between subject and predicate, nor that between preciseness and ambiguity of reference, nor that between things and persons and states of things and persons, nor that between self-referring expressions and expressions which are not self-referring.

Roughly speaking, truth-value changes thought not to affect propositional identity are those which are, as we say, "due to the passage of time." But sentences in which time, or a particular time, is the subject do not express propositions having alterable truth values. Sentences which explicitly mention specific times, and not with words like "now" or "today," also do not, in general, express propositions having alterable truth values. Most examples given of propositions with alterable truth values are either (a) propositions expressed by sentences having words like "today" and "yesterday" or "now" and "then" as their subjects, or (b) propositions expressed by pairs of sentences uttered at different times but with unchanged tense. Because the tense has

not changed we know that the (one particular) thing that was asserted to hold of someone is being asserted still to hold of him. Or to put it another way: what was implied to hold of a thing or person at a certain time—the time of utterance, say—is implied later to hold of it or him at another time—the time of the second utterance. Any change in truth value can be here equally described either as due to the passage of time, or as due to a change in (not of) the subject.

Why say, though, that there is one proposition having different truth values? One reason for believing that propositions can change their truth value is the existence of expressions like "Before his accident it was true to say of him that he was quite fearless, but now . . ." and "It is no longer true that so-and-so and such-and-such." But these expressions, which admittedly do imply that propositions change in truth value, can be translated very easily into expressions which imply just the opposite: for example, the expression "It is no longer true that X is the leader" can be translated into "X, who was once the leader, is now no longer the leader" or into "X was the leader and now X is not the leader" or even into "At time t, X was leader and at time t_1 X was not the leader." So ordinary language does not give a quick answer to the question. The most it shows, or at any rate seems to show, is that either way of thinking about propositions is at least possible. It makes it seem probable that neither way of thinking about propositions is actually self-contradictory.

Another reason for believing that propositions can change their truth value is the repugnance of saying that the identity of what is said remains unknown until one has decided whether or not what is said is true or false—a view that appears to follow from the denial of the changeability of truth values of propositions. For if it is absolutely impossible for a proposition to change in truth value, it looks as if its being true or false, as the case may be, is one of its essential features. So the question "What proposition is this?" would always contain the question "Is this proposition true or false?" which manifestly leads to absurdity, since one has to know what a proposition is before one can ask if it is true or not. It is obviously true that we can know what

a proposition is without knowing its truth value; otherwise the question "Is what he said true?" could never be asked.

On the other hand, the belief that difference in truth value is a sufficient condition of nonsynonymy as between two propositions is surely not actually a self-contradictory one.

If a difference in truth value as between two propositions p_1 and p_2 is a sufficient condition of p_1 and p_2 being two separate propositions, does it follow that in order to know that two sentences express one and the same proposition it is either the case that (a) one must know that the truth value of the proposition expressed by the one sentence is in fact thus, and that the truth value of the proposition expressed by the other sentence is in fact thus, too (that is, one must know the actual truth value of each proposition), or (b) one must know that the truth values of the propositions expressed by the two sentences are the same without necessarily knowing what this truth value in fact is?

There is no reason to insist that knowing that two sentences express the same proposition involves knowing that the truth value of the proposition expressed by one sentence is in fact T (say), and that the truth value of the proposition expressed by the other sentence is in fact also T, unless this is necessarily involved in knowing that the truth values of the propositions in question are the same. Is it, then, possible to know that the truth value of p_1 is the same as the truth value of p_2, without knowing what in fact this truth value is? The answer is certainly yes, since what the truth value of a proposition is depends on what the proposition is about, and what it says about it, and of what time (if it is not a timeless proposition) it is true (and so on), and cannot vary independently of these things. Thus if one knows what things, events, places, speakers, times, and so forth, are referred to, mentioned, or implied to be relevant by two propositions, and knows that they are all identical, then one also knows that the truth value of the propositions, whatever it is, is in fact the same for each proposition. Identity of reference, predicate, time referred to (where relevant), place referred to (where relevant), speaker involved (where relevant), and so for all the things that might be relevant, is a sufficient condition of identity of truth value. (It is still an open question as to whether

this in turn involves identity of the two propositions.) What counts as relevant is any item of which it could in certain circumstances be said that it was a source of ambiguity.

If one knows that the references made by two propositions or the times referred to, and so on, are not identical, then one knows not, of course, that their truth values are different, but that they might be different. Consequently, it is possible to know that the truth values of p_1 and p_2 are the same without knowing what the truth value in fact is, but not possible to know that p_1 and p_2 have different truth values without knowing what the truth value of each is. Consequently the view that a difference in truth value is a sufficient condition of nonidentity of propositions does not involve the assertion that to identify a proposition one must know its truth value, and thus is not an absurd view (though it might be false for all that).

If it were false that a difference in truth value as between two propositions was a sufficient condition of nonidentity, it would follow that one and the same proposition could be true and false at different times. Thus the dictum that a proposition entails itself would either be false or would need some addition. There are here three alternative possible additions:

(1) "At time *t*, *p* entails *p*." (2) "*P*-in-so-far-as-it-is-uttered-at-time-*t* entails *p*-in-so-far-as-it-is-uttered-at-time-*t*." (3) "*P*-in-so-far-as-it-refers-to-or-implies-something-about-time-*t* entails *p*-in-so-far-as-it-refers-to-and-so-forth-time-*t*."

(1) The first version is plainly false, since it means merely that an entailment holds at a certain time between *p* and *p*, which is not true if *p* is the name of something that can be true and false. The idea that entailments only hold sometimes is of course a repugnant idea in itself.

(2) The second version presents a transmogrified account of the definition of a proposition in terms of utterances. For it implies that what entailments hold between are not propositions but utterances. It separates the truth-bearing character of propositions from the implying and entailing character. Is this really necessary or plausible, however? Do we really have to find out when something was said before assenting to the view that that something entails itself? This seems to me much more objec-

tionable than saying that we need (often) to find when something was said in order to clear away certain kinds of ambiguity about what that something that was said in fact was.

Also, this version introduces confusion into the notion of entailment, for timeless propositions do not need this restriction, so that entailments hold between any utterance of, for example, "Water runs downhill" and any other utterance of "Water runs downhill" (if it holds between utterances at all, that is) but only between a particular utterance of (say) "Tom has gone" and *the very same utterance*—though this hardly makes sense. So "A proposition always entails itself" would mean two pretty different things.

(3) The third version introduces an unnecessary muddle about the distinction between sentences and propositions. The relationship between the proposition and a particular time *t* is made to mirror the relationship between the sentence and, for example, a particular person, thing, event, place, or time. This relationship is external. "The things a sentence can say or be used to say" do not serve to identify it as a sentence. They depend on what things, persons, events, places, times, and so forth, are referred to when the sentence is used in a given context or type of context, and what properties and relationships, including spatial and temporal ones, are attributed to them. To speak of a proposition "in so far as it refers to time *t*" is to imply that it can refer to other times, and thus to speak of it as if it were a sentence. (Nevertheless, there are occasions on which it is useful to consider the proposition as partly similar to a sentence in this respect.)

Must we add "at time *t*" to all sentences? This is like asking, must the thought of ambiguity be with us always? The truth is that we must be prepared to make what has turned out to be ambiguous less so. The words "at time *t*" or something similar can be added to an extremely large number of sentences. Just as well. This does not mean it is always necessary to do so.

To sum up: is a proposition an objectionable metaphysical entity? The answer to this is: some of the reasons for introducing the notion of proposition, though humdrum, are good; therefore the notion so introduced is not an objectionable metaphysical entity.

Are the criteria of identity for propositions too numerous? And what criteria have been in fact suggested? The criteria of identity of sentences and of utterances are not those of propositions, although it is the case that propositions cannot be examined and identified unless sentences and utterances are examined and identified.

The criteria of identity of propositions cannot be equated with criteria for the identity of meaning straight off, because "identity of meaning" may mean too many things. It may mean identity of reference and of predicated properties, or of verification, or of audience reaction, or of use, or kind of use; or it may mean mutual entailment; and so on.

No account of identity in terms of reference, verification, or use will cover all possible kinds of example, for a reason which I shall mention below. The account in terms of "audience reaction" is incoherent. Accounts in terms of mutual entailment presuppose that certain questions concerning truth and falsehood have been settled.

Is identity of truth value a necessary condition of propositional identity? Some ways of talking suggest that the right answer to this question is yes, others that it is no.

Other considerations also appear to point in different directions. Hence we can at least say that neither way of thinking about propositions is actually self-contradictory.

Some conclusions can now be drawn. First, whatever entailment may be (apart from material implication), identity of reference is a general condition of mutual entailment, for though it does not (of course) guarantee mutual entailment, no general guarantee of truth-value identity can be given without it. Second, in the case of non-necessary propositions, the notion of identical verification often gives sense to the notions of identity of reference and identity of predicated properties (which however also in turn give sense, in a way, to the notion of identical verification). Third, although "kind of use" is a difficult concept, it is important in so far as it is one involved in showing nonidentity between those examples of those types of expression which have no entailments—the nonidentity, for example, of a command and a question.

Thus it is not right to say that the criteria of identity of prop-

ositions are too numerous, since many of them are very closely connected to each other.

Fourth, finally, there are two distinct ways of thinking about propositions, or two notions of propositions, even after questions, commands, and the like have been put to one side. One allows changing truth values and one does not. The first is not susceptible of definition in terms of entailment. We can define it, though, in terms of the sort of thing it covers. Roughly speaking, the first notion of proposition covers (a) some things which are said to people (see p. 509, for example), (b) things said which are described as things said about or of people or things or which, as we say, are true or false of or about this or that. The second notion of proposition is typically in operation, as it were, when the proposition is described as the thing that was said. The thing that was said entails itself and is either true or is false but does not change. When one thinks of those propositions called necessary, or those called timeless, one thinks of them in this way. When one considers entailment and implication, one thinks of propositions in this sense of the term proposition. Great numbers of sentences which "state accomplished facts," or which purport to do so—for example, most of those in history books—express propositions which we think of in this way. Sentences whose subject terms refer to times also express propositions thought of in this way, for times do not change with time.

"Something said *about* so-and-so" can generally be, as it were, turned into "something *that* is said, full-stop." For example, "In 1900 *S* was *P*, but now it is no longer true of *S* that it is *P*" (which makes us think of the proposition as that which has become false) can be turned into "That *S* was *P* in 1900 is true (is a true proposition) but that *S* is still *P* is false (is another, false, proposition)." It seems to me that it would not be possible to submit, for example, any "timeless propositions" to the reverse process and that it would be peculiar even to try. This seems to me further to be a reason for saying that the primary notion of propositions is the one in which a proposition's truth value does not change.

JENNY TEICHMANN

Somerville College

DISCUSSION

STRAWSON'S AUDITORY UNIVERSE

IN his recent book *Individuals* P. F. Strawson introduces a universe of sounds and uses this device to highlight certain points concerning our own and other possible conceptual schemes. I would like to examine this chapter for I feel that both Strawson's procedure and his conclusions are suspect.

I

This auditory universe is introduced in the chapter on "Sounds" which is concerned with the question "Is the status of material bodies as basic particulars necessary in any scheme which provides for objective particulars?" In the previous chapter, "Bodies," Strawson has dealt with the importance of material bodies as the basic particulars in our own conceptual scheme; he now wishes to go on and discover what is necessary for the existence of a class of basic particulars in any nonsolipsistic conceptual scheme. And so he invents this auditory no-space world in which it might be possible to indicate basic particulars without using a spatiotemporal framework such as we use in our own world.

Thus, in order to discover something about the nature of our own conceptual scheme—whether it is contingent or necessary that material bodies be the basic objective particulars— Strawson suggests not so much an alternative conceptual scheme as a different conceptual scheme in a very different but still conceivable world. To this extent he is no longer doing descriptive metaphysics, as he calls it, which "is content to describe the actual structure of our thought about the world,"[1] nor is he doing revisionary metaphysics, which "is concerned to produce a better structure"[2]; instead he is doing what we might call "comparative metaphysics," which deals with alternative schemes in different worlds in order to cast light upon our own scheme and our own world. And I think this change of procedure is of crucial importance since Strawson can no longer be justified in certain assumptions he makes: something may be perfectly true of our own conceptual scheme, and so an important and essential point

[1] P. 9.
[2] P. 9.

for descriptive metaphysics, and yet not at all true of all possible conceptual schemes (that is, it will be a contingent rather than a necessary truth about our present scheme).

Now there are two objections I want to make against the content of this chapter. One of these will lead us to examine his assumptions and procedure in more detail, and I think both these objections arise because Strawson has not been sufficiently careful in making this transition from descriptive to comparative metaphysics.

The first point is that Strawson is unwarrantedly misleading in the way he describes this auditory world. He asks two questions: "(1) What are the most general statable conditions of knowledge of objective particulars? (2) Do these most general conditions involve the requirement that material bodies should be basic particulars, or is this simply a special feature of our own scheme?"[3] And since it is the spatio-temporal framework that requires that material bodies be the basic particulars, the quest is to invent a no-space world (for example, an auditory one) which will nevertheless provide for the category of objective particulars. If he can invent such a world then he has shown that the concept of space is not essential for a nonsolipsistic conceptual scheme; if he cannot, then he has done something toward showing that space is essential, although we may be tempted to try other no-space worlds; and if—and this is what does happen—he can, but only by means of a special device, then we should compare this device with our present concept of space, and see how the two differ or are alike. It would seem that it is justifiable to conclude that the features which are common to both are those features which any nonsolipsistic conceptual scheme will have to exhibit—that if our present concept of space is not necessary, then at least something very like it is. One might remark that to discover what is necessary we would have to try all possible no-space worlds, because some of them may not need devices so closely resembling our concept of space as is the auditory device. But Strawson introduces this Master Sound, this auditory device, not because of the nature of the particular no-space world he is investigating, but because he is looking for the possibility of objective particulars. So it seems we will meet precisely the same difficulty and have to offer similar devices as solutions, no matter what type of no-space world we invent. Certainly it seems that Strawson has hit upon the most profitable universe to use as an example.

But the point is that Strawson should be trying to do without

[3] P. 62.

space, or any equivalent for it, as much as possible. Now the Master Sound is introduced as a means of reidentifying particulars, and not as a means of making the auditory world parallel to our familiar one. It is by considering our own world that Strawson decides that it is something like the spatial dimension "housing" particulars which is needed for the reidentification of objective particulars, but we should not let this lead us to think that the basic aim in introducing the Master Sound is just to reproduce an analogy to space in auditory terms. If we did this altogether, we would be creating not a no-space world but a quasi-space world. In fact the pitch dimension turns out to be very like our familiar spatial one, but this is because certain features of the spatial dimension are apparently indispensable for our purpose, and not because we want to provide simply an auditory analogy to space. In fact the less like space our pitch dimension turns out to be the better.

Now Strawson does speak as if he wished to reproduce our familiar dimension in auditory terms, but what would be the point in this? We have asked, "To what extent is the spatiotemporal framework necessary in a system which provides for objective particulars?" and our question is answered when we discover whether we have to introduce an analogy for space and, if we do, how like space that analogy is. If Strawson speaks as if the aim of the auditory universe were simply to reproduce our own in analogical terms (this would be a rather fruitless part of descriptive metaphysics) rather than to cast light on what is essential for any nonsolipsistic conceptual scheme, then he makes himself liable to objections which are actually irrelevant. Someone might say, "Your analogy to space is unlike space in the following ways: it is not a good, accurate, complete one," and in fact Strawson does consider such objections. But when we look upon the pitch dimension as some sort of alternative for the spatial one, then points of this sort are not so much objections to what we say as rather illuminating indications of those aspects of our concept of space which are not essential to all nonsolipsistic schemes.

To proceed to the second objection: throughout this chapter and the one preceding it, Strawson appears to assume that the existence of the concept of a reidentifiable particular entails, and is in turn entailed by, the existence of the concept of an object existing unperceived (these two will henceforth be abbreviated to RIp—the concept of a reidentifiable particular—and EUPp—the concept of a particular which exists unperceived; the assumption is then that RIp= EUPp, that to have the concept of one is to have the concept of the

other). On pages 71, 72, and 73 we find ample evidence that Strawson holds that these two entail each other.

This assumption leads Strawson into difficulties, and it is, I think, an unwarranted one. At the end of this chapter he notes that, though RIp is necessary for the concept of an objective particular (hereafter symbolized as Op), it is not sufficient for that concept. Hence it seems we might tabulate the relationships between these three concepts in this way:

$$\text{Op} \rightarrow \text{EUPp} = \text{RIp}.$$

But obviously EUPp → Op. Perhaps we might form the concept of an objective particular which nevertheless does not exist while we are not observing it, but surely if we talk of something existing unperceived, this means that the particular in question must be an objective one. This entailment however, turns the above diagram into this:

$$\text{Op} = \text{EUPp} = \text{RIp}.$$

And this means that once we have allowed for the concept of a reidentifiable particular we have also allowed for the concept of an objective particular; that is, the conceptual scheme in question is not a solipsistic one. But Strawson has rejected this conclusion, and it seems prima facie that the two most objectionable implications embodied in the above diagram are RIp → Op and RIp → EUPp, although the opposite implications seem admissible.

But Strawson accepts these implications, or at least he explicitly accepts RIp→EUPp, which is the root of the problem; to discover why, we have to turn back to the first chapter where Strawson was doing descriptive metaphysics, stating truths about our own conceptual scheme rather than stating things which would be true of any and all possible conceptual schemes. On page 35 he has this to say: "A *condition* of our having this conceptual scheme is the unquestioning acceptance of particular identity in at least some cases of non-continuous observation," and with this we feel inclined to agree. Then he goes on to say that the man who refuses thus to reidentify at least some particulars is not casting doubt on what we all say, but is rather refusing to say what we all say. The skeptic, according to Strawson, does not quarrel with what we say in our scheme; he rejects it and suggests we replace it with another. He is in effect a revisionary whom we may listen to, but whom we need not follow.

Now the important thing to notice is that all Strawson is saying at this point is that we must allow for the reidentification of particulars

after a break in observation *if* we are to have a scheme such as we do have. He definitely does not say that this is the only possible sort of scheme, or even that all nonsolipsistic schemes must allow for such reidentification (this latter point might be expressed: "Does Op always imply RIp?" and an answer will be suggested in what follows). But more important still is that Strawson does not say that the particular thus identified must exist during the break in observation, but only that we must be able to reidentify at least some particulars after this break; that is, he is not saying RIp → or = EUPp!

And so, although he may have dismissed one sort of skepticism (namely that which doubts any claim for identity after a break in observation), he has not rejected the idealist who holds that an object exists only when perceived; and if the idealist is to be described as some sort of skeptic, as seems natural, then Strawson's characterization of the skeptic will not do.

Thus it seems that Strawson has nothing positive to support RIp → EUPp either in this conceptual scheme or in any other, solipsistic or not, and it seems that we can argue positively against such an entailment. Obviously a solipsistic scheme might well deny it (for example, one which characterized all headaches as parts of one and the same headache), and several nonsolipsistic schemes might do so, too (for example, one established in a world very like our own except that some objects vanished and reappeared intermittently; then we could have criteria very like our present ones for saying "one and the same *x* again," even though there were no spatiotemporal continuity, and thus we could reidentify the particulars without asserting that they continued to exist while unobserved, because they would not even exist all the time they were observed!). Finally we have the idealist and the phenomenalist who propose variant forms of our own conceptual scheme which differ from the standard form precisely in that they do deny this implication. So Strawson's transference of something which is debatable in this world to a different scheme in a different world, with the aim of discovering something about all possible schemes of a particular sort, is, to say the least, suspicious.

We therefore reject this implication RIp → EUPp. We may in fact accept it in our familiar scheme, but this in no way entitles us to carry it over into comparative metaphysics as a general truth. This rejection will help Strawson out of one particular problem. In the chapter on sounds he mentions that although Op → RIp he is not certain that the converse implication holds, that although RIp is necessary for Op he is not sure that it is also sufficient. "Although it

might appear obvious that it was a sufficient condition"[4] he does not think it is, and he goes on to sketch an argument which does appear to lead from RIp to Op (and we have already noted how he appears to be forced into accepting that these two entail or equal each other). First, RIp → EUPp ("For the concept of a reidentifiable particular was held to entail that of a particular existing whilst unobserved") and, second, EUPp → Op ("and hence in general the distinction between being observed and not being observed, or at least some analogous distinction,"[5] and this is all that is necessary for a non-solipsistic scheme), and so RIp → Op. But having sketched this proof Strawson goes on to show how we could have the concept of a reidentifiable particular without having that of an objective particular, yet he does not review the proof or point out any mistake in it! Of course we would deny that RIp has to entail EUPp, that because we can reidentify sounds we have to characterize them as existing even when not heard, and surely Strawson would want to deny this rather than EUPp → Op.

It seems, then, that this rejection of RIp → EUPp can be used to support Strawson's conclusions, for he can now go on with a clear conscience and state that although RIp is a necessary condition for Op, it is not a sufficient one. And so we have two modifications of Strawson's position: (1) we reject RIp → EUPp, and note that if we accept it we also accept RIp = Op; (2) we modify Strawson's characterization of the skeptic as a revisionary rather than as a man who quarrels with part of the scheme we use.

II

We have thus made two general objections to the content of this chapter, both of which are raised in the first place because Strawson has not been sufficiently careful in passing from descriptive to comparative metaphysics. As we proceed we will find that these points and their consequences seem to put the chapter into a rather dubious position, and suggest a different approach to the problems raised in it. First, however, let us notice a further consequence of Strawson's own conclusions which follows from the rejection of RIp → EUPp.

Strawson seeks to invent a scheme which allows for reidentifiable objective particulars without having material bodies as these basic particulars, but because he equates RIp with EUPp he sets his stand-

[4] P. 81.
[5] P. 81.

ards higher than is necessary and so ends up with something stronger than he realizes. His Master Sound pitch dimension is introduced as a means of giving particulars places where they might exist unobserved, and thus it allows for both EUPp and RIp. Strawson believes that these are equivalent, but we have seen that the first is considerably stronger than the second and even implies Op. If this is so Strawson's auditory world must allow for Op as well as EUPp and he has thus made it capable of supporting a nonsolipsistic scheme, whether it be phenomenalistic ("There is to be found a rather nice melody two octaves higher") or not ("A rather nice melody exists at this moment just two octaves higher"). And indeed Strawson appears to realize this when he outlines the argument mentioned above.

Strawson appears to think that EUPp is not sufficient for the establishment of a nonsolipsistic conceptual scheme, and this would involve him either in denying EUPp → Op or in saying that Op is not sufficient for the establishment of such a scheme. But surely the concept of an objective particular, as opposed to and hence implying the existence of a class of subjective particulars, entails the distinction, which Strawson finds so important, between observer and what is observed; and surely this distinction in turn entails the concept. It seems that the existence of such a concept is precisely what is postulated in saying that the scheme in question is nonsolipsistic. As for the other alternative, we have already seen good reasons for holding EUPp → Op, and surely the concept of a thing existing while we are not observing it does imply that a distinction has been made between the self and its states and what is neither of these; surely talk of things existing while I am not there entails talk of things other than myself. How then does Strawson reject this argument?

What he does is invent a technique whereby we can talk about phenomena met with in the auditory universe without using either the concept of myself-as-observer or the concept of particulars which are observed. This he does quite easily and so concludes, "For the description of the universe so far given, we do not appear, if we follow the rule, to need to make any use of the distinction between oneself and what is not oneself."[6] Now the crucial word here is "need"; what Strawson does is show that the conceptual scheme he has developed (one which allows for EUPp and Op just as our familiar one does) is not the only possible conceptual scheme which might be used to describe the invented universe. His technique, his rule, is simply a

[6] P. 83.

way of setting up a solipsistic conceptual scheme in that world. And this shows not that the auditory universe does not allow for a nonsolipsistic scheme, but that it does not necessitate it. But our concern is with what is essential for the establishment of a nonsolipsistic scheme, not with what, if anything, forces us to adopt a scheme of a certain sort.

III

Now Strawson's original question was "Can we conceive of a conceptual scheme which is like ours in that it provides for a system of objective and identifiable particulars, but is unlike ours in that material bodies are not the basic particulars in the system?" and it seems that we have arrived at an affirmative answer, but only by making the sound particulars rather like material objects in some ways. That is, to do this, although he does not consider himself successful, Strawson found it necessary to postulate a second dimension to that of time—one which made it possible to talk of particulars as existing at the same place on one dimension (the temporal one) and yet at a different place on the other. Apparently what we need for a nonsolipsistic conceptual scheme is the idea of "*places* at which those sounds are audible, but these are places at which I am not now stationed."[7] In other words what is necessary for Op is (1) a dimension other than the temporal one and (2) the idea of points other than that which is being observed at the moment; or, more briefly, the idea of a dimension which extends beyond the bounds of present observation, apart from the temporal dimension. If we have the idea of such a dimension, then we can talk of points along it and particulars, existing at those points, which are not now observed.

So in attempting to discover what characteristics a consciousness must exhibit if it is to be capable of supporting a nonsolipsistic conceptual scheme, our question has become "What is necessary for the idea of such a dimension?" To answer that it seems we must first ask what a dimension is, and what is necessary for the idea of any dimension whatsoever.

Now the concept of a dimension is one we use to order phenomena met with in the course of experience, so that we can identify particular phenomena and relate them to others of a similar sort, not with any reference to particular characteristics which they may or may not share, but with reference to their place in the course and character of experience in general. First we need to be able to discriminate partic-

[7] P. 74.

ulars, and second we need to be able to fit the occurrence of these particulars in the course of experience into the over-all context of, that experience, to characterize them not by the features they display in themselves but *by the place they occupy in an order* which many different particulars share. For example, if the particulars met with in consciousness demonstrate any change whatsoever we get an order of succession. For change is always change from something to something, and we can characterize particulars met with by reference to the place they occupy in this one-way order of change. Obviously to characterize a particular in this way, and so relate it to all other particulars which are met with in the course of such change, is to fixate it in some sort of temporal dimension. To have dimensions x, y, and z is to have three ways of relating certain phenomena by means of recognizing orders which all phenomena of that sort display. And to form the concept of a dimension, to be able to relate different particulars in terms of the order in which they are met, we have to notice such orders occuring in the course or content of consciousness. For example, we notice that in the course of consciousness there occurs change, and event follows event in that change, and so we use the concept of "the order of succession in which things happen" to relate event to event. Again we notice that different particulars may be met with at the same instant, and that they are ordered in a certain way (in fact there are an infinite number of ways in which we might describe the order of the contents of our visual field, although three are all that are necessary to characterize the elements in terms of their relationships to each other): for example, they occur in a certain recognizable order from side to side, and so we use this order to relate particular to particular by reference to the point in that order at which they are to be found. We may refer to dimensions as "dominant ordering relationships" if we remember that this order by means of which we fixate and so relate the particulars is not established by introducing the concept of a dimension. The order is already evident in our experience, and we apply the concept of dimension to those orders which are useful and most convenient for referring to particulars.

Dimensions, then, are established by noticing ways in which phenomena encountered in experience are ordered and by using this order as a means of referring to and relating the phenomena. But how do we establish the idea of a dimension which extends beyond the present content of experience? Apparently we need to be able to talk of an order in which particulars occur which extends beyond what is observed at the present moment, and to do this we need to have

noticed that the particulars occur in that order and *recur* in that order, so that although we are not now aware of them we are quite able to say that they will appear, and in such and such an order. It is this observed recurrence of particulars in a certain order which enables us to reidentify both places and particulars, for reidentification depends mainly upon recognition within a context. We must first of all be able to characterize certain particulars as in some respects, if not all, "similar" to those we have met before. Moreover, we need to be able to characterize the content, the setting, the order in which we meet them, the relationships which they have with other particulars, as in some important respects if not in all "similar" to that in which we encountered the first set of particulars. And such changes as there are, whether in the particular itself or in its relationship to other particulars, must be explained in terms of the observable characteristics of the particular in question, and in terms of the time elapsing between the two observations. If these three requirements are met (the third requires considerable elaboration but it should suffice to indicate the general sort of thing required) then we are in a position to reidentify the particulars as the same again. If we can reidentify the particulars then we may also characterize them as occurring in the same order, as occupying the same places in that order, and so on. However, we notice that even exact similarity of particulars and context does not seem to force us into characterizing them as one and the same.

Therefore if our consciousness includes a recognizably repetitive change in its content, then we can reidentify not only the particulars met with in the course of this change, but also stages in the change itself (points in that order in which the particulars are encountered), and since not all particulars or all stages are present at one time, we may talk of a particular associated with a certain point in the order, a certain stage of the change, when neither this particular nor this point are being observed. In other words we can talk of a particular as being at a place of which we are not now aware. And what makes this talk possible, although it does not make it unavoidable, is the awareness of a change in what is experienced (so that it is possible to talk of things not now experienced) which is, however, repetitive in some way or another (so that it is possible to reidentify particulars and so reidentify places).

What, then, are the necessary conditions for EUPp, Op, and RIp? It seems that all that is necessary for these concepts is the existence of rules which tell us when and how to characterize different particulars

as being of the sort in question. In fact, to put the matter crudely, all that is necessary for any concept whatsoever is the existence of a rule by applying which things are characterized as having that concept applicable to them, for concept and the rule by which we apply it amount to much the same thing. When we set up a rule for describing particulars in a certain way, then we have formulated that particular concept. But the crudity of this statement should not affect the important point—that what is necessary, and all that is necessary, for us to characterize a particular as one which exists when unperceived, as an objective particular, or as one and the same particular again, is some sort of rule which we can use to characterize the particular in question as being of one or other or all of these sorts. To reidentify particular P we need to have rules which enable us to say that P_2 is the same particular as P_1 and so on.

But according to this description even a consciousness in which nothing is perceived as being similar to anything else could allow for the reidentification of particulars. For example, a consciousness consists of a number of different visual sensations—first, a black square on a white background; next, an expanse of blue; next, a dazzling flash; next, a vertical line running through the visual field, and finally an indefinite blur. According to what has been said we could even reidentify particulars in this universe, just so long as we set up some rule for this reidentification: for example, "every second discriminable element is actually a recurrence of the same particular again," that is, there are two rapidly changing particulars succeeding each other. Apparently even this consciousness could support a conceptual scheme which allowed for RIp and perhaps even Op, if we are prepared to talk of the order in which these two "basic particulars" are encountered.

This conclusion only seems absurd because it is not at all clear what is meant by saying that such a conceptual scheme is *possible* in such a world. By this we do not mean (1) that such a scheme is likely to be adopted in such a universe, nor (2) that a being with such a consciousness, who does not have prior knowledge of a universe and a conceptual scheme like our own could invent such a scheme, nor (3) that such a scheme would be of any use in such a world. Now Strawson asked "Could there exist such a conceptual scheme?" which he equated with "Can we make intelligible to ourselves the idea of such a scheme?"[8] and if by this he means "Can *we* invent such a scheme in

[8] P. 60.

such a world?" then the answer is the obvious affirmative which we have given. This tells us nothing, however, about our own conceptual scheme except that we can translate parts of it to other, more "primitive" universes, and we can understand such schemes in such universes only because we already understand our present scheme in our own world. But Strawson adds the qualification "I shall provisionally interpret the question 'Can the conditions of knowledge of objective particulars be fulfilled for a purely auditory experience?' as meaning 'Could a being whose experience was purely auditory, *make use of* the distinction between himself and his states on the one hand, and something not himself or a state of himself—on the other?' "[9] So it seems that what we are looking for are not the conditions for understanding any sort of conceptual scheme in any sort of world, but rather the conditions which make the particular scheme *useful* in the particular world, and this is a very different question from that with which we have so far concerned ourselves.

IV

So it appears that all that is necessary for us to have a nonsolipsistic conceptual scheme in a no-space world is that we already have a nonsolipsistic conceptual scheme in this world (hence perhaps Strawson's talk of introducing the spatial analogy). But the important question is really "What is necessary for a being to develop or make use of such a conceptual scheme in such a world?" Now by this we might be asking:

1. Is such a scheme possible in such a world? Can *we* invent a nonsolipsistic scheme in the no-space world?
2. Is such a scheme possible in such a world? Could the inhabitant of such a world invent such a scheme?
3. Is such a scheme useful in such a world? Could *we* put such a scheme to use in such a world?
4. Is such a scheme useful in such a world? Could the inhabitant of such a world put the scheme to use?

The answer to 1 has already been given. Since all we need for a scheme which allows for Op is a rule for calling a particular objective, all we need to do is adapt the rule we have in our own world to this no-space world, and once we can discriminate particulars we can, however paradoxically, characterize them as existing independent of the perceiver. Questions 3 and 4 appear to depend upon something

[9] P. 69, my italics.

rather different, namely on what we use the scheme for, rather than the nature of world or of scheme. In our own case (in answer to question 3) we might say that whether we adopt a solipsistic or a nonsolipsistic scheme depends on which is the easiest to formulate and work with; if one is simpler than another it will apparently be the more useful, although it may not necessarily be. And as we have seen, Strawson outlines both sorts of scheme for use in the auditory world he invents, and of the two the solipsistic scheme may well be the simpler. However, to obtain an affirmative answer to both 3 and 4 all we need to do is add some further requirements, some nonessential, perhaps psychological reasons why we should talk in one way rather than another.

The answer to question 2 would depend on similar considerations —the intelligence of the inhabitant, the need for any sort of conceptual scheme, and so on. And so it appears that the existence of a conceptual scheme of a particular sort depends not on the nature and contents of the consciousness, as Strawson seems to think, but on the needs and nature of the possessor of that consciousness. For instance, a solipsistic scheme appears to be absurd, even though it may perhaps be possible, in a world such as ours where the conceptual scheme and language have been formed in the first place for the purpose of communicating with other people.

V

Strawson asks two basic questions: what are the most general statable conditions of knowledge of objective particulars and do these most general conditions involve the requirement that material bodies should be the basic particulars? He admits himself that these turn out to be questions "about the extent to which, and the ways in which, *we* might find it possible to reinterpret, within a part of our experience, some of the most general conceptual elements in our handling of experience as a whole."[10] And he later comments, "There remains a doubt about the *meaning* of saying that we have here a possible reinterpretation of the idea of an unperceived and hence of a reidentifiable particular. What are the tests for whether it is a possible reinterpretation or not? I do not think there is any test beyond what we find it satisfactory to say. One can certainly influence the finding by pointing to respects where the parallel holds

[10] P. 63.

or fails to hold—and can also suggest improvements. But no more."[11] Our own conclusion—that *we* can say what we like in these worlds —seems to be rather like this. Provided that we can discriminate particulars, we can, however artificially, reidentify them and characterize them as existing objectively. The question becomes rather "What is the point in this translating of our concepts to these worlds?" and certainly the more like our own world these worlds are, the more point there seems to be.

It is indeed hard to see what light the invention of the auditory world throws upon our present conceptual scheme. The question whether *we* find this reinterpretation possible turns out to have a most unilluminating answer, and the general conditions of knowledge of objective particulars seem to lie in how and why we form a particular conceptual scheme, rather than in what the scheme is used to describe. Even the Master Sound, which provided the analogy for space and made much of the reinterpretation so satisfactory, proves superfluous in that it would be possible to do without it. For example, a tune is heard, an easily recognizable tune, which begins to fade after a time, but before it fades completely a new tune is heard softly, gradually growing louder, although like the first tune it may stay at a particular level of loudness for some time. When it fades a third tune is heard, and so on. If the sound particulars do not overlap we have a break in consciousness. Now after a while the order in which tune replaces tune is found to repeat, and so we can establish a dimension along which we can fixate the individual sound sequences. "Where is 'Honeysuckle Rose'?" "It is two tunes past 'Sweet Sue,'" and so on. In fact to use the Master Sound as a means of reidentifying tunes we had first to reidentify parts of the Master Sound itself.

The Master Sound's importance lies mainly in acting rather like the observer's body in our own world, as Strawson points out, and because it can and does move from particular to particular along the dimension it provides a standard means of measuring along a dimension not possible in a world were there is no such element. That is, it is possible by using the Master Sound to characterize two particulars by reference to the distance between them without mentioning other particulars, by reference to the amount and rate of change in that element alone. It might be felt that this analogy for motion should be a requirement for setting up the concept of a dimension, but I do not think this is so for dimensions are possible

[11] P. 81.

without motion (for example, in a sense datum), and there seems to be nothing analogous in the case of time unless it be the artificial standard change measured by clocks and timepieces.

So it appears that the auditory world, as described by Strawson, is little more than an indication of how it is possible to reduplicate features of our own experience within certain limited parts of that experience, and it is not at all clear what light this throws upon the way our present conceptual scheme is used to describe our experience, or how and why this sort of scheme was originally developed.

DON LOCKE

University of Canterbury, New Zealand

HOBBES'S "TABLE OF ABSURDITY"

HOBBES's linguistic theories have in recent years provoked a good deal of discussion and admiration.[1] Critics are discovering in Hobbes "clear and precise" insights into "semantical matters,"[2] "remarkable anticipations of modern ideas,"[3] "techniques of logical analysis,"[4] and a host of other things. Although this is certainly not doing Hobbes's reputation any harm, it is not the Hobbes we know who always emerges out of these discussions. Hobbes's curious attempt at a "Truth" Table in Chapter V of *De Corpore* is a case in point.[5]

The prevailing interpretation of Hobbes's Table is that it is an attempt to supply a much-needed logic for evaluating philosophic propositions.[6] Hobbes ("who was remarkably sensitive to the philosophical dangers of falling into nonsense"[7]), the advocates of this view argue, wished to show that his predecessors had committed absurdities by being "insensitive to the logical behaviour of different classes of words"[8] and unaware that language may be "systematically misleading." So he pointed out that people often mixed their categories in making propositions and that what they needed, above all, was a "Theory of Absurdity" to guard against such misuse of language. In this Hobbes seems to have "anticipated modern techniques of logical analysis." The scheme he fashioned to do this work was a crude one but it was an effort in the right direction.

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[1] See, for example, Håkan Törnebohm, "A Study in Hobbes' Theory of Denotation and Truth," *Theoria*, XXVI (1960), 53-70; R. M. Martin. "On the Semantics of Hobbes," *Philosophy and Phenomenological Research*, XIV (1953), 205-211; Gordon Hostettler, "Linguistic Theories of Thomas Hobbes and George Campbell," *Review of General Semantics*, III (1945), 170-180.

[2] Martin, *op. cit.*, p. 211.

[3] *Ibid.*, p. 205.

[4] Richard Peters, *Hobbes* (London, 1956), p. 136.

[5] The texts of Hobbes's works used in this paper are those edited by Sir William Molesworth, *The English Works of Thomas Hobbes* (London, 1839-1845), hereafter referred to as *E.W.* Quotations from *Human Nature*, however, are taken from Tönnies' edition (*The Elements of Law Natural and Politic*, Cambridge, 1928) and will be abbreviated to *El. of L.* The Table is given in *E.W.*, I, 58.

[6] See Peters, *op. cit.*, pp. 135-137, and Stuart Hampshire, *The Age of Reason* (New York, 1956), p. 46.

[7] Hampshire, *op. cit.*, p. 46.

[8] Peters, *op. cit.*, p. 136.

Although this highly complimentary description of Hobbes and his intentions manages, perhaps, to isolate and rescue what is from our present point of view most valuable in Hobbes, what I should like to show here is that from a historical point of view it tends to falsify Hobbes and misrepresent his intentions. For the Table, when examined closely and in context, reveals that Hobbes seems to have regarded it in quite a different light and was not so "sensitive" as his recent critics would have us suppose.

To say this is not to deny, of course, that Hobbes believed that the misuse of language is responsible for a good deal of the confusion that abounds in philosophic speculation, and that in order to achieve clarity of thought we must free our minds from the confusions which arise out of words themselves. No doubt Hobbes says this, but he says it in terms of his system as a whole. Hobbes's "Table of Absurdity," that is to say, is not an isolated element in an otherwise connected system of thought: neither in *De Corpore* where it is given nor in Hobbes' system as a whole does its appearance on the scene startle the reader. Yet its all too apparent crudity does. I should like to begin by considering these two points.

An examination of Chapter V of *Human Nature* (1640), of Chapters IV and V of *Leviathan* (1651), and of Chapter III of *De Corpore* (1655) suggests that the Table grew out of Hobbes's persistent efforts to classify all possible types of propositions.[9] He had apparently arrived at the view that there are two large classes of propositions: factual or empirical propositions grounded on sense experience and analytic or arbitrary propositions grounded on reason.[10] But although Hobbes's analytic propositions deal merely with the way we use symbols and their logical implications and are thus devoid of factual content, he nevertheless wished to maintain that they are neither senseless nor absurd. And it was important for him to maintain this, for analytic propositions, according to him, lie at the very basis of deductive science. He firmly believed that the first principles of deductive science or reasoning are definitions or statements of the meaning of names. And "names" are given arbitrarily.[11] Geometry ("the only science that it hath pleased God hitherto to bestow on mankind"[12]) is such

[9] To say nothing of the discussion in Chapter V of *De Corpore* immediately preceding the Table where he speaks of "errors of sense and cogitation" and "errors which . . . proceed . . . from reasoning amiss."

[10] *E.W.*, III, 71; *El. of L.*, pp. 18-19.

[11] *E.W.*, III, 52; *E.W.*, I, 37, 84, and 388.

[12] *E.W.*, III, 23-24.

a deductive system of thought, yet it is productive of knowledge, "for when we calculate the magnitude and motions of heaven or earth, we do not ascend into heaven that we may divide it into parts, or measure the motions thereof, but we do it sitting still in our closets or in the dark."[13] But although reason and its products are mere "fictions,"[14] these fictions are not plays of the imagination, and are to be sharply distinguished from a different class of fictions or third class of propositions, those productive of absurd results.

To define more sharply these three distinct types or classes of propositions, Hobbes proposed a system of predication appropriate to them. Generally speaking, he believed that "error" should be predicated only of sense experience (that is, "when a man reckons without the use of words"); "truth" ("falsehood" and "absurdity") should be predicated only of the second group of propositions; "absurdity" (and "falsehood") should be predicated only of the third group. And he argued that when dealing with particular things, only "error" (and not "falsehood" or "absurdity") is the possible danger, but when making a "general assertion, unless it be a true one, the possibility of it is unconceivable. And words whereby we conceive nothing but the sound, are those we call absurd, insignificant, and nonsense."[15] And because analytic propositions, which Hobbes has in mind here, can lead only to truth, he sometimes uses "falsehood" and "absurdity" interchangeably, and implies that these predicates apply strictly only to the third kind of proposition.[16] With this scheme of predication in mind Hobbes proceeded to define, in a systematic way, the nature of those propositions whose fit predicate is "absurdity" and produced his Table.

The general background of the Table thus indicates that it did not grow out of some independent desire to arrive at a "Theory of Absurdity," but rather out of a desire to consolidate his position in regard to the two types of knowledge to which alone, he thought, we have a valid claim: factual knowledge expressed in empirical propositions and hypothetical knowledge expressed in analytic prop-

[13] *E.W.*, I, 92.

[14] See his *Third Set of Objections*.

[15] *E.W.*, III, 32.

[16] It would perhaps have been more consistent to say that "truth" and "falsehood" are fit predicates of the first group (that is, when our "reckonings" are verbalized); that neither "truth" nor "falsehood" is a fit predicate of the second group (since the entire structure is analytic and arbitrary); and that "absurdity" is a fit predicate only of the third group.

ositions. A consideration of its contents reaffirms this impression.

Names, Hobbes argues, fall into four separate classes: names of bodies, names of accidents, names of phantasms, and names of names. Absurdities arise when one class of names is used as if it belonged to another class. If in a proposition we combine a name belonging to one class with a name belonging to another class, we leave the bounds of logic, for "that proposition only is true, in which are copulated two names of one and the same thing; and that always false, in which names of different things are copulated."[17] The only way to speak with reason and logic is to make propositions which consist of parts, all of which originate from the same class—that of bodies, or of accidents, or of phantasms, or of names and speeches. On the basis of these general principles he constructed his Table, showing the seven different ways in which one must arrive at absurd propositions.

1. If the name of a Body		the name of an Accident
2. If the name of a Body	be	the name of a Phantasm
3. If the name of a Body		the name of a Name
4. If the name of an Accident	copulated	the name of a Phantasm
5. If the name of an Accident		the name of a Name
6. If the name of a Phantasm	with	the name of a Name
7. If the name of a Body, Accident, or Phantasm		the name of a Speech

The first thing that strikes one about this table is that it is oddly asymmetrical. One would expect six ways in which absurdity is inevitable ("names" and "speech" assimilated), or ten ("names" and "speech" taken separately), or nine ("names" and "speech" taken separately but no absurdity following from their copulation), but not seven. If this table was designed, as has been suggested, strictly as a "technique of logical analysis," one might have expected Hobbes to be more precise himself.

Some clue as to his real intentions, however, may be gathered from his introductory remarks to his Table where he lists, first, only four basic elements out of which the Table is constructed—"bodies," "accidents," "phantasms," and "names"—and then adds this curious note: "It may happen, also, that the name of a *body*, of an *accident*, or of a *phantasm*, may be copulated with the name of a *speech*. So that copulated names may be incoherent seven manner of ways." Obviously

[17] *E.W.*, I, 57.

the fifth element, "speech" (or the seventh way), came to him as a kind of afterthought. What had Hobbes forgotten? Simply another element of the logical scheme? Surely not, for in that case he would have developed it properly and in relation to the other parts of the Table. What he had forgotten, I should like to suggest, was the very thing which prompted him to construct the Table in the first place —the business of propositions—which, it suddenly occurred to him, could itself be brought (not very well, as his lame explanation of this last type of absurdity a few pages later shows) under this schema.[18]

His last statement in his account of the Table lends further support to this view. He says, in conclusion: "The falsities of propositions in all these several manners, is to be discovered by the definitions of the copulated names."[19] This is a perplexing statement to make at this point in the argument, for one had come to believe that the falsity or absurdity of such propositions followed inevitably from their copulation. If their absurdity does not follow inevitably from their copulation how do they differ from ordinary propositions, ones in which names of the same class are copulated—the truth or falsity of which, he states in the very next sentence (of the following paragraph), is to be determined in precisely this way? But what Hobbes probably means by "definition" is simply the classification of terms according to the five basic elements of the Table: bodies, accidents, phantasms, universals, and propositions. That these five elements happen to be the five basic units of his philosophy as well is surely no accident.

These two facts about Hobbes's Table—its place in the line of his argument and its obvious limitations—seem to suggest that the Table arose out of nonlinguistic considerations and was meant to serve an extra-linguistic purpose. Precisely what these other considerations were becomes clear when one examines Hobbes's exposition of the Table, an exposition which reveals at once how intimately the entire scheme he fashioned is connected with his system and echoes its doctrines.

I will now give, first, a synopsis of Hobbes's exposition of the Table, and then proceed to show how the entire scheme grows out of his system and was meant to serve as its schematic plan.

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[18] After completing his explanation of this last type of absurdity he promptly proceeds to forget about it and continues to speak only of names of bodies, accidents, names and phantasms. Obviously he had not quite made up his mind about the status of this element in his Table.

[19] *E.W.*, I, 61.

1. The absurdity of copulating abstract names (the name of an accident) with concrete names (the name of a body):

Examples: "A *body* is *extension.*"
"*Whiteness* is a *white thing.*"

Explanation: "For seeing no *subject* of an *accident* (that is, no *body*) is an *accident*: no name of an *accident* ought to be given to a *body*, nor of a *body* to an *accident.*"

2. *Examples:* "*Colour is the object of sight, sound of hearing.*" "*Space* or *place is extended.*"

Explanation: "For seeing . . . light, colour, sound, space, etc. appear to us no less sleeping than waking, they cannot be things without us, but only phantasms of the mind that imagines them."

3. *Example:* "*Universale est ens.*"

Explanation: "For *genus*, *universale* . . . are names of names, and not of things."

4. *Example:* "*An object is of such magnitude or figure as appears to the beholders.*"

Explanation: "For the same object appears sometimes greater, sometimes lesser, sometimes square, sometimes round."

5. *Example:* "*The definition is the essence of a thing.*"

Explanation: "For definition is not the essence of any thing, but a speech signifying what we conceive of the essence thereof."

6. *Example:* "The *idea of anything is universal.*"

Explanation: "As if there could be in the mind an image of a man, which were not the image of some one man."

1. Hobbes's exposition of the first type of absurdity is clearly an extension of his position in regard to the whole question of universals. The same kind of absurdities which arise from the misunderstanding of the function of universal and abstract names is shown here to arise from the improper combination or copulation in a proposition of names of bodies with names of accidents, or vice versa. Hobbes thought that the great function and value of abstract and universal names lay in that they help us to achieve a certain economy of thought by allowing us to reason about things without being constantly

anchored to objects. Abstract names, however, have no counterparts in the external world. They are merely ways in which we account for and distinguish those names which do have counterparts in the external world.[20] They constitute a kind of second series or order referring to a first order and not to that to which the first order directly refers. Their great abuse lies in thinking that there exist real counterparts in the external world which answer to these abstractions or constructions. Their value, therefore, can be maintained only so long as one remembers that these names are merely terms descriptive of other terms (thinking-*body*, motion *of*, *the* life *of*, and so forth) and are in no sense self-supporting. To say, then, that "extension is a body" or "whiteness is a white thing" is to think of "extension" or "whiteness" (being names of accidents) as being names of bodies in their own right. And just as "accidents" are "the manner of our conception of body"[21] and it would be a gross error to materialize such accidents as if they had a separate existence in the outside world, so it would be absurd to combine the name of an accident with the name of a body.

To put Hobbes's exposition of the first type of absurdity in this wider context is to see it in its proper perspective. By making it a rule of language never to mix these two classes of words Hobbes seems to have thought that he had succeeded in producing a final test of the truth of his position in his polemic against the "Aristotelians" who maintained a doctrine of essences.[22] The Table, in this first form, constituted in his eyes a statement of his philosophic position on this question of the objective validity of universals, and he seems to have regarded this statement (with what justice is, of course, another question) as additional proof of that position.

2. His explanation of the second type of absurdity indicates that he regarded its function in a very similar light—and a much more revealing light. We commit absurd speech here when we combine names of bodies which have a real existence in the external world with names of phantasms which are products of our reactions to stimuli coming from without us. The weakness of his argument is fully evident here, for his examples of absurd speech of this type—unlike the proposition "Whiteness is a white thing"—are not ones whose absurdity is self-evident. There is nothing obviously absurd

[20] *E.W.*, III, 26.
[21] *E.W.*, I, 104.
[22] *E.W.*, III, 70.

about the proposition "Space or place is extended." Aware of this himself, he is led to try to establish the absurdity of this type of proposition by appealing to an extra-linguistic argument. In this case his argument is drawn from his general views regarding sensible qualities, the objectivity of which, here again, he had taken such great pains in *Human Nature*, as elsewhere, to deny.[23] His views on this subject and their difficulties are too well known to require further expansion here.

3, 4, 6. Hobbes's discussion of absurd speech of types 3 and 4 adds nothing new either to his linguistic doctrine or his philosophical position. He does not have too much to say about these forms of absurdity. And there is good reason for this, for the third type of absurdity results in such propositions as "There are things universal" and he has already exhausted this topic in his discussion of absurdity of type 1; and the fourth type of absurdity results in such propositions as "An object is of such magnitude or figure as appears to the beholders" and he has exhausted this topic in his discussion of absurdity of type 2. His short notes of explanation and examples, however, do manage to show just where his emphasis really lies. In short, absurdity of types 1 and 3 is based directly on his Theory of Universals, and absurdity of types 2 and 4 is based directly on his Theory of Perception.

Absurdity of type 6, on the other hand, is based on both theories and follows from their basic premises. He stated his view on this matter succinctly in the *Leviathan* when he declared: "One universal name is imposed on many things, for their similitude in some quality, or other accident: And whereas a proper name bringeth to mind one thing only; universals recall any one of those many."[24] If, therefore, the imagination (as his Theory of Perception presupposes) provides us only with individual determinate imagery and only the name (as his Theory of Universals presupposes), not the image, has a general use, then to combine in a proposition a name of a phantasm (an image) with the name of a name (a universal) is to commit (as his sixth type of absurdity declares) absurd speech.

5, 7. In 1, 2, 3, 4, and 6, Hobbes seems to have been intent on showing that certain types of names have no counterparts in the external world and should therefore not be copulated in a proposition with those names that do have such counterparts. In 5 and 7 he seems

[23] *El. of L.*, p. 5.
[24] *E.W.*, III, 20.

to be intent upon showing, mainly by implication, that such second-order names can result in meaningful speech when not so copulated. In both cases, however, the final appeal is to the body of his philosophy.

It is absurd, Hobbes argues in 5, to say that "definition" is the essence of any *thing* but that of a speech. The definition of *X*, for example, is not the essence of *X;* the definition of *X* is a speech "signifying what we conceive of the essence" of *X*. To accept such speeches as true or meaningful, he implies, is to leave the door open to occultism. To avoid it, however, is to point the way to rationalism of the type he himself has proposed, for to Hobbes propositions made up of universal names and propositions made up of abstract names perform an important function in language and are not empty of meaning. The meaning they contain, however, has been arbitrarily arrived at. Reason, which consists in connecting words according to the conventions which we ourselves have arbitrarily decided about their meanings, cannot, it is true, give us knowledge of the nature of things, yet it can and does express the most essential properties of things (for example, our numbering system). Our linguistic system, although arbitrary (made by ourselves), is also meaningful. And it is this aspect of his thought which Hobbes here wishes to reinforce.

His exposition of absurdity of type 7 was much too long to quote, but what he says is this: "Necessary," "contingent," " by itself," and "by accident" (like "definition") are not names of things but of propositions or speeches, and have meaning when confined to the description of propositions. It is absurd, therefore, to combine them in a proposition together with names of things and say, for example, that "some things exist necessarily or by themselves, others contingently or by accident." What deceives people here is that they think that there is more than one source of imagery (and that, therefore, "one idea should be answerable to a name, another to a proposition") when there is only one—our fancy which receives sensory stimuli from the external world. But although there is only one source of imagery, there is (and here again this is his main point and the *raison d'être* for bringing in, at the last moment, this additional fallacy) another source of knowledge, "propositional knowledge." This type of knowledge is not to be confused with empirical knowledge, since it is not about "things" but rather about the correct use of words in propositions, the different kinds of which he has isolated in the body of his philosophy.[25]

[25] See *El. of L.*, pp. 18-19, where he puts this much better.

I think I have now sufficiently laid bare the pattern of Hobbes's argument. The question now is, does that pattern suggest that Hobbes was trying to prove something about language? I think not, for if Hobbes had really meant to prove that people committed absurdities in, say, the matter of the "objective" existence of secondary qualities or the "objective" validity of universals because they were unaware that language may be systematically misleading, he could hardly have hoped to do so by appealing, as he does, to the facts of his philosophy, for his opponents could then have easily pointed out to him that he was arguing in a circle. Is his Table, then, founded upon a *petitio principii*, or is there some alternative explanation which will save Hobbes from this embarrassment? I think there is, and it consists simply in seeing that what Hobbes really wished to do was not to prove some new thesis about language, but rather to lend added strength to a position already established by subsuming it under some general logical linguistic scheme. For Hobbes's line of argument does not proceed from language to the body of his philosophy, but from the body of his philosophy to certain facts about language which tend, he thinks, to lend support to that body. The ultimate appeal is philosophical, not linguistic. And Hobbes's obvious and repeated reference to the body of his philosophy in his exposition of the Table seems to indicate that it was the desire to consolidate the gains made in that philosophy that ultimately prompted him to construct the Table.

To sum up: Hobbes's "Table of Absurdity" is an attempt to lend added weight to a body of thought by subsuming it under a general schematic plan having the sanction and authority of language. It shows or proves nothing about the logic of language that that body of thought has not already assumed to have proven and, therefore, discovers nothing about language that is not already presupposed by that philosophy. If it is a "technique of logical analysis," then that technique is suitable only to Hobbes's philosophy, or to a philosophy which accepts its presuppositions.

To read the Table in this way is, I fully realize, to take the glamor out of it. But one ought to keep in mind, however, that if Hobbes was as sensitive to the logical behavior of different classes of words as has been suggested, and if he had arrived at any clear conception regarding the dangers of mixing one's categories, he would have seen that his own system suffers throughout of the same weakness, and would have been led to revise his thoughts on a good number of points. That he did not seems to indicate that whatever discoveries

he made regarding the "logic of language" arose out of his attempts to consolidate his gains rather than out of any independent insights, and that he was too deeply committed to those gains to be able to appreciate the importance of his discoveries in any disinterested light.

S. MORRIS ENGEL

University of New Brunswick

PROFESSOR GOODMAN'S PUZZLE

In a recent article in the *Philosophical Review*[1] S. F. Barker and Peter Achinstein attempt to deal with a certain puzzle that Nelson Goodman has presented in his book, *Fact, Fiction, and Forecast.*[2] This is the well-known paradox about "grue" and "bleen." I believe that Professor Goodman's reply, in the same issue, does succeed in answering the objections of Barker and Achinstein on the ground that they do give a certain priority to what they call the blue-green language. However, the question is still open, and I think there is a better argument that can be used to show that Goodman's attempt to raise a puzzle is not, in the end, successful.

Goodman attempts to raise this puzzle in his discussion of what he calls "the new riddle of induction." The old problem of induction he calls "Hume's problem," i.e. the problem of "justifying induction"; and he dissolves it in a way which has become standard. I have no interest in this problem and no desire to resurrect dead issues. Nor am I directly concerned with the broader aspects of the new problem of induction, which Goodman takes to be the problem of defining confirmation, i.e. defining the relation between an evidence statement S_1 and a hypothesis S_2 which it confirms. This reduces to the problem of what kinds of hypotheses can be confirmed, which reduces in turn to the question of what kinds of predicates can be projected from examined to unexamined cases, or as Goodman calls them, "well-behaved" predicates. One move that is often made is to say that we can distinguish a class of so-called purely qualitative predicates. Other predicates are to be called "positional" in that their definition requires some reference to a particular thing or to some particular location or position in time or space. Qualitative predicates are then said to be projectible and positional predicates not, and it is claimed that some of the puzzles about confirmation arise from trying to project positional predicates.

The puzzle can be stated in a preliminary fashion thus: Let us take the predicates "blue" and "green." Intuitively these seem to be the

[1] "On the New Riddle of Induction," *Philosophical Review*, LXIX (1960), 511-522.

[2] Published by the Athlone Press, London, 1954.

sort of predicates that one would want to call purely qualitative. Using these predicates, Goodman then defines another pair of predicates "grue" and "bleen" which we would intuitively not regard as qualitative but as positional. However, he believes he can show that there is an exact symmetry between the two pairs of predicates such that if one pair is excluded as positional and therefore not projectible the other pair must be excluded as well. So we have the following dilemma: either reject both pairs of predicates as positional or accept both as qualitative. It is a dilemma because the first alternative goes against our intuitions: we do want to have "blue" and "green" as projectible and well-behaved predicates; and the second alternative leads to inconsistencies because it is quite easy to show that if both pairs of predicates are projectible the same evidence will confirm contradictory hypotheses. Goodman avoids the dilemma by dispensing with any attempt to define a class of qualitative predicates, and says that the projectibility of a predicate is a matter of its "entrenchment" in the language. He then goes on to develop a somewhat elaborate theory of how predicates become entrenched in a language. I am not concerned with this theory, or even with the problem of defining a class of qualitative predicates. I believe this can be done, but I do not know exactly how. It is an interesting problem, but it is not my problem. However, I will presuppose that if any definition of such a class excludes the predicates "blue" and "green" or includes "grue" and "bleen" it is *ipso facto* an unacceptable definition. I will presuppose further that virtually all native speakers of English know what "blue" and "green" mean.

So much for preliminaries; now to a statement of the puzzle: "... the predicate 'grue' ... applies to all things examined before [a certain time] t just in case they are green but to other things just in case they are blue."[3] Further, "... the predicate 'bleen' ... applies to [all things] examined before time t just in case they are blue and to other [things] just in case they are green."[4] The paradox arises from the fact that at time *t*, whatever evidence we have for the hypothesis, say, that all emeralds are green is equally good evidence for the hypothesis that all emeralds are grue. But if these hypotheses are assumed to hold beyond *t*, it turns out that an emerald that is grue after *t* is blue and therefore not green. Now the crux of the puzzle is that if we say that "grue" and "bleen" are not purely qualitative

[3] *Ibid.*, p. 74.
[4] *Ibid.*, p. 79.

predicates because they are explained by means of a specific temporal term, we run into the following difficulty:

> True enough, if we start with 'blue' and 'green', then 'grue' and 'bleen' will be explained in terms of 'blue' and 'green' and a temporal term. But equally truly, if we start with 'grue' and 'bleen', then 'blue' and 'green' will be explained in terms of 'grue' and 'bleen' and a temporal term; 'green', for example, applies to emeralds examined before time t just in case they are grue, and to other emeralds just in case they are bleen.[5]

So we appear to have an exact symmetry between the two pairs of predicates of such a kind that both of one pair are qualitative (or positional) if and only if both of the other pair are qualitative (or positional).

A more precise statement of the interdefinability of the two pairs of predicates is in order. For one thing, it might prove a source of confusion that in defining his predicates Goodman uses the word "examined," as though a thing's being examined were logically necessary to ascribing "grue" or "bleen" to it, since its being examined would be, as it were, a part of the meaning of the predicates. This complication is unnecessary to stating the puzzle as Goodman wants to state it. We should say more simply: a thing is grue at a certain time if it is green at that time and the time is prior to t, or if it is blue at that time and the time is after t. So I adopt the following conventions: Goodman's specific time t will be designated by t_0. I adopt a time variable t; and the following diadic predicates are defined in an obvious way: *GExt*, *GUxt*, *BUxt*, *BExt*. For example, *GExt* is true of the ordered pair x_i; t_i if and only if x_i is green at t_i. Finally, I adopt the dyadic predicate *Ptt'*, true of t_i; t_j if and only if t_i is a time prior to t_j. The following four schemata give us a relation of symmetry:

(1) $(x)\ (t)\ (GUxt \equiv : GExt.Ptt_0.\mathrm{v}.BUxt.Pt_0t)$
(2) $(x)\ (t)\ (BExt \equiv : BUxt.Ptt_0.\mathrm{v}.GExt.Pt_0t)$
(3) $(x)\ (t)\ (GExt \equiv : GUxt.Ptt_0.\mathrm{v}.BExt.Pt_0t)$
(4) $(x)\ (t)\ (BUxt \equiv : BExt.Ptt_0.\mathrm{v}.GUxt.Pt_0t)$

Actually, a more precise statement would require eliminating the two different kinds of variables, perhaps by adopting predicates, "... is a time," and "... is an object." With this qualification, it will be obvious that the conjunction of (1) and (2) is equivalent to the conjunction of (3) and (4). It would seem that such an equivalence is sufficient to support the claim that there is an exact symmetry

[5] *Ibid.*, p. 79.

sufficient to make it impossible to distinguish between the two pairs of predicates in point of qualitativeness or positionality. But is it sufficient? I think it can be shown that whatever symmetry has been established is quite irrelevant to Goodman's case.

The crucial mistake comes when Goodman says that if we start with "grue" and "bleen" we can define "blue" and "green" in terms of them and a temporal predicate. But what can he possibly mean by "starting with" in this context? This phrase is less innocuous than it might seem. We know quite well what it is to start with "blue" and "green" and define "grue" and "bleen": we know what "starting with" means here because we know what "blue" and "green" mean; since we can inspect something and tell whether it is blue or green and can also determine our present temporal location, so to speak (in whatever way this is determined), we then have a way of deciding whether a thing is grue or bleen. Since we have given a precise definition of "grue" and "bleen," they have become predicates of our language; we have adopted them; they mean exactly what they were defined to mean and nothing more. But the matter is not so simple when we talk about "starting with" "grue" and "bleen" and "getting back to" "blue" and "green." In what sense do we already know what "grue" and "bleen" mean well enough to define "blue" and "green" in terms of them and a temporal term? Only in the sense that we have already defined "grue" and "bleen." But then the interdefinability is trivial, since all we have done is retrace our logical steps and got back where we started. So that while there is a formal symmetry, there is also an asymmetry resulting from the fact that we know what "blue" and "green" mean independently of any definition.

The argument up to this point will no doubt seem somewhat simple-minded to those familiar with the issue, so more remains to be said. It might be replied, for example, that a formal symmetry is all that is necessary to establish Goodman's case, that talking about "knowing the meaning" of the predicates in question is wasted breath unless we can also *say* what they mean, since we then have no way of arguing that it would be impossible for someone to "know the meaning" of "grue" and "bleen" independently and then define "blue" and "green" in terms of them and a temporal term. Formally, it could be said, "blue" and "green" are what we might call the undefined primitives (or dummy predicates) of (1) and (2), and "grue" and "bleen" are the dummy predicates of (3) and (4). Or one could say simply that they are all dummy predicates, their actual "meaning"

being irrelevant. Any further statements we may make about which predicates we already "know the meaning of" independently is simply a statement of fact about the language, about what predicates happen to be entrenched in the language, and such a fact could be otherwise.

However, I claim that this reply will not bear careful examination. It will not do because formal relations do not hold between predicates but between dummy predicates, predicate letters; because the identity of a predicate is not to be established solely with reference to its orthography (a string of letters) or with reference to a string of sounds (phonemes), and therefore formal relations are irrelevant. The point of these remarks will, I hope, become clear shortly. For the moment, let us note that this sort of reply does admit something: that in order to raise the puzzle it must be conceivable that there be creatures who could independently know what "grue" and "bleen" mean in the way we know what "blue" and "green" mean. It could not be otherwise. For when we talk about "starting with" "grue" and "bleen" one of three situations must obtain: *(i)* We already know what they mean because we have given them a meaning in terms of "blue" and "green." But this is inadequate for the reasons already given. *(ii)* "Grue" and "bleen" are just dummy predicates and nothing more, utterly devoid of meaning. But if so, and if we define some other dummy predicates in terms of them and having the same orthography, and so on as our English predicates "blue" and "green," they will be just as devoid of meaning and will have only an orthographic or phonetic connection with the English predicates. Questions of projectibility would then not arise, since it is predicates which are projected, not dummy predicates. So we are led to *(iii)*, viz., the hypothesis of creatures who have a quite astonishing ability to look at a thing and say straight off whether it is grue or bleen without first determining whether it is blue or green and what their temporal location is. It is not simply that the hypothesis of such creatures is a device for raising the puzzle in an interesting and forceful way, but that it is abolutely essential to raising the puzzle at all.

But how very curious such a hypothesis is! We thought that "grue" *meant* green before t_0 or blue after t_0. But now we are assuming that there might be creatures who could know that a thing is grue or bleen and not know whether it is blue or green; indeed they might not even know what "blue" and "green" mean, might not be able to identify blue and green things, might not be able to learn how to use such words without a definition in terms of "grue" and "bleen." In fact, it is obvious that there could be no such creatures. Let there be no

mistake about what I am claiming. My argument here is not that there could not be any such creatures because they would have to be capable of direct intuitions of time or some such thing. I mean that the supposition that there could be creatures who could tell whether a thing is grue or bleen without first determining whether it is blue or green and their present temporal location, is unintelligible. It is unintelligible for the simple reason that "grue" and "bleen" have already been defined: they are already predicates in *our* language; and what can, as it were, be appropriated by any hypothetical creatures is not the predicates themselves, but the sequence of letters or phonemes associated with them. All we can presuppose are creatures who use certain predicates which are phonetically or orthographically identical with our predicates "grue" and "bleen," and which *might* have some other connections as well.

However, the argument is not finished, since there is still one move that can be made. Consider the following argument:

"Very well," it might be said, "this shows that any predicates that we find our hypothetical creatures using can't just be identified without further ado with our predicates 'grue' and 'bleen' on the basis of orthographic or phonetic similarity. However, we might still identify two such pairs of predicates in the following way: Suppose we find our creatures using predicates which we represent as 'glef' and 'brid.' Suppose that after observing their verbal behavior we come to the conclusion that they apply these words to just those things which are grue and bleen respectively. Suppose finally that they, after observing our verbal behavior, decide that we apply the predicates 'blue' and 'green' to just those things which are blif and gred respectively. ('Blif' means brid before t_0 or glef after t_0; 'gred' means glef before t_0 or brid after t_0. 'Blif' and 'gred' are part of their language in exactly the way that 'grue' and 'bleen' are part of ours.)"

Let us suppose that this actually comes about. Now the question is: What have "glef" and "brid" got to do with "grue" and "bleen" and what have "blue" and "green" got to do with "blif" and "gred"? The implication of the question is that in order to support Goodman's case it is necessary to establish a symmetry between "blue" and "green" and "glef" and "brid." No other symmetry will do the job. The problem is then whether we can establish interconnections among these eight predicates in such a way that "blue" and "green" will be qualitative (on any definition of qualitativeness) and therefore projectible if and only if "glef" and "brid" are. The difficulty is

that these two pairs of predicates belong to different languages spoken by different creatures.

In order to deal with the matter effectively it is necessary to try to be a bit imaginative and specify in more detail just what it is that we have supposed, in the preceding two paragraphs, to have come about. Let us call our creatures Saturnians—personally I am tired of Martians. We will suppose that we came in contact with them and learned their language and they learned ours (or at least so we and they supposed) before t_0. The syntax of their language is sufficiently like ours so that we can mark off a class of predicates in their language much like our class. Since t_0 lies in the future we will most likely have translated the predicates "glef" as "green" and "brid" as "blue," and there is so far no problem. Two things could then happen. At some time before t_0 the Saturnians begin to assure us that at t_0 everything that is glef will change to brid and vice versa. We smile indulgently at this quaint superstition and go about our business. Much is our surprise then when t_0 arrives and they inform us in all seriousness that these changes actually have taken place, and, not only that, they talk as if such changes really did take place. (Let us call this situation 1.) Or, what could happen is that at t_0, without any warning, the Saturnians start behaving in very extraordinary ways, much as we might behave if radical and totally unexpected changes started occurring in our world. Later they tell us that on that historic occasion, t_0, everything that was glef suddenly changed to brid and vice versa. They eventually recovered from the shock, they tell us, and have adapted to the change. This will be situation 2. In either situation we noticed no changes and begin to wonder. And they, since we have apparently failed to notice obvious changes, begin to wonder as well. We each have the problem of accounting for the other's behavior. How are we going to account for their behavior? It is impossible to say in advance—there is probably more than one hypothesis that we could come up with. But if we exclude the hypothesis that the Saturnians are clever and consistent liars or that it is all a huge joke, then we would probably adopt some hypothesis to the effect that Saturnians are capable of detecting certain characteristics of things that we are not capable of detecting (possibly even that they have an extra "sense" that we lack). I find no absurdity in supposing this or any difficulty in understanding it.

On the basis of this hypothesis we conclude that our original translation of "glef" and "brid" was wrong and that we are, at least at present, unable to learn their meanings. The only way we are

able to tell whether a thing is glef or brid is to ask a Saturnian. So we invent the predicates "grue" and "bleen" to have a convenient way of talking about the extension of "glef" and "brid" and we define them in the usual way. We adopt this only tentatively, however, since we cannot be absolutely sure that these changes, which unfortunately we cannot detect, will not occur again. In situation 1 we can ask the Saturnians if the changes are expected to happen again, and, assuming that the answer is no, we will base our adoption of "grue" and "bleen" for this purpose on their testimony. With situation 2 we can only assume that such changes will not happen again.

The objection that is most likely to be made here is that meaning is essentially a matter of extension (which is not to say, of course, that the meaning of a predicate *is* its extension) and therefore, it is wrong to suggest that we cannot learn the meanings of such words because we cannot detect the relevant characteristics. This, it might be said, is like the old philosophical worry that everyone might see qualitatively different colors even though we all apply the same color words to the same things. This sort of objection cannot be sustained. The question is, would we as native speakers of English regard "grue" and "glef" as meaning the same, given the hypothesis that we have adopted to explain the odd behavior of the Saturnians? The answer is obviously no. For suppose we did. Then to say (as every Saturnian does) that a thing has changed from glef to brid at t_0 is to say that it has changed from grue to bleen at t_0. But to say that a thing has changed from grue to bleen at t_0 or vice versa is to say that it has "changed" either from green to green or from blue to blue, i.e. that it has not changed at all. We have then destroyed our hypothesis for we had assumed that things did change (for the Saturnians)—specifically from glef to brid. If it is argued that this only reduces the whole hypothesis to unintelligibility, I can only reply that I have no difficulty in understanding it, and there is no other kind of hypothesis that can even begin to make Goodman's case plausible (given the fact that the possibility of our Saturnians whose behavior is roughly as described is necessary to stating the case).

The issue can now be stated as follows: Can we establish a symmetry between "glef" and "brid" and "blue" and "green" in such a way that whatever grounds there might be for saying that "blue" and "green" (or "glef" and "brid") are purely qualitative (or positional) apply as well to "glef" and "brid" (or "blue" and "green")? The problem is thus seen to be more complicated than it was in Goodman's original statement, since there is no way of defining either pair of

predicates by means of the other. They belong to different languages. Speakers of the one language can never learn the meaning of the relevant predicates in the other. Another complication is that questions of the projectibility of the predicates would not arise until after t_0. We can then ask two questions: whether the doubtful predicates were projectible before t_0 and whether they are projectible afterwards. But no matter which question we raise about "glef" and "brid" we can only answer it by simply asking the Saturnians—in effect, we are never in a position to project such predicates; only they can do so because of the very untranslatability of the predicates into our language. The answers they will give to our questions will depend on whether situation 1 or 2 occurred. Given situation 1, we will first ask them how they knew that the changes would occur. Suppose they were able to tell us about some well-established physical theory of theirs on the basis of which they successfully predicted the change. Then we could say that before t_0 it would not have been legitimate to project "glef" beyond t_0, much as if we had a physical theory to the effect that emeralds change color after they reach a certain age; the projectibility of "green" with respect to emeralds would then be limited by theoretical considerations. Further, if the Saturnians tell us that on the basis of their theory they expect no more changes, we will have to regard "glef" and "brid" as projectible from t_0 on.

We need not consider situation 2 since it would not fundamentally change the conclusion to be drawn. The obvious conclusion (which, in effect, I have already drawn) is that the projectibility or not of "glef" and "brid" has no connection whatever with the projectibility or not of "blue" and "green." It is obvious because evidence for the glefness or bridness of something is not evidence for the blueness or greenness of it, and so we could not possibly get contradictory results by trying to project the two pairs of predicates from examined to unexamined cases.

KENNETH SMALL

University of Chicago

BOOK REVIEWS

THE FOUNDATIONS OF MATHEMATICS. By EVERT W. BETH. Amsterdam, North-Holland Publishing Company, 1959. Pp. xxvi, 741. $13.25.

This book discusses a wide variety of topics in logic and the foundations of mathematics. It begins with some historical remarks (Part I), followed by a discussion of the axiomatics of various branches of mathematics (II), basic topics in symbolic logic (III-IV), the existence of mathematical entities (V), the paradoxes (VI), further topics in logic (VII-VIII), and finally some remarks on the philosophy of mathematics (IX).

The book contains a staggering amount of material; it includes most of the "classical" results of mathematical logic and various further developments leading into quite esoteric topics. For example, most of the material of Kleene's *Introduction to Metamathematics*,[1] itself a 500-page book, is in one way or another included, along with much else. The book also contains quick introductions to abstract algebra and topology. The main purpose of such comprehensiveness is to improve understanding between mathematical logic and philosophy, which has suffered from the ramification and specialization of logic and the fact that the philosophical purpose of the results is less clear-cut.

Unfortunately the survey itself is not very well adapted to its purpose. The comprehensiveness means that the treatment is far less detailed than in such works as Kleene's. This lessens the value of the book both for the logician and for the nonlogician. The logician knows many of the theorems and wants either to see the proofs in full detail or to find rich references to other related results; but Beth's exposition is too compressed and lacking in elegance to be the best for a beginner, and the beginner will not learn from Beth the habits of precision which are so important in logic. On the other hand, so much of the space is devoted to logical materials that the philosophical remarks, which are the real *raison d'être* of the book, are rather sketchy.

We should expect that in a work of this sort the choice and arrangement of materials would be guided by philosophical problems and principles. In fact, the whole of Part VII and many other items

[1] New York, Toronto, Amsterdam, and Groningen, 1952.

scattered through the book have no visible relevance to philosophy. Beth seems to include many results of his own without regard to whether his general purpose calls for their inclusion. He emphasizes semantical and algebraic-topological methods. The latter were developed with a view to bringing logic closer to other branches of modern mathematics rather than to philosophy. It is puzzling that Gödel's incompleteness theorem, which is vital to Beth's viewpoint, is put off from its natural place in the exposition (ch. 10 or 11) to chapter 21.

We should point out some mathematical obscurities and errors. On page 131, the "definitions by recursion" of (5) are *primitive* recursive definitions, while the "recursive definition" of (6) can be any *general* recursive definition.

On page 209, the proof of (12) requires a proof of the equivalence of U and $\bar{U}$.

On page 597, the proof that Peano's arithmetic P is undecidable is incorrect, since Beth neglects the possibility that the decision function is too complex for the resulting consistency proof to be formalizable in P.

On pages 247-248, the proof of the consistency of the system Q requires for its completion a direct proof that the formalization of elementary predicate logic of section 76 is equivalent to the system of semantic tableaux (section 67), since the consistency proof assumes formal proofs to be semantic tableaux, while Q was originally formulated with the logic of section 76. This is not a trivial omission, since the equivalence presupposes Gentzen's theorem (in the form of p. 278[2]) and is the real meat of the consistency proof.

This error is part of a treatment of finitary theory of proof which gives a quite inadequate idea of it. This subject is an outcome of Hilbert's proposal to justify the methods of argument of classical mathematics by formalizing them and proving the consistency of the resulting system by methods which not only do not presuppose the actual infinite (that is, are "constructive") but which use only inductive definitions and proofs of an elementary, though not exactly specified, type. Beth, however, writes as if finitary methods were a somewhat pedantic curiosity. He gives up Hilbert's methods before reaching

[2] Roughly, the theorem says that if the inference from A to B can be formally proved, and likewise from B to C, then the inference from A to C can be formally proved. This is nontrivial only because of the special character of Beth's semantic tableaux and Gentzen's systems shorn of cuts.

the main results obtained by them, Herbrand's and Gentzen's theorems.[3]

It seems to me that what is involved here is a question not simply of clear exposition or of filling gaps in proofs, but of a basic confusion on Beth's part on the relation between subject matter and method in metamathematics. This confusion is to some extent fostered by the Hilbert school itself, since they tie the use of finitary methods to the thesis that nonconstructive methods are doubtful as mathematics. A deeper reason, however, is that logic naturally splits into the study of the formal properties of definition, inference, and proof, and the study of the relation of these to the (presumed) extralinguistic reality which mathematics describes. The former requires for its epistemological significance some close relation to, say, proofs before our eyes by which we know mathematical truths. Then it is natural to require that proofs asserted to exist be obtainable by effective methods,[4] since a "proof" in Plato's heaven is not a contribution to our knowledge. The concepts which would be used in nonconstructive proofs in this domain would not have the significance which gave the original concepts their importance.

The latter study, semantics, is essentially nonconstructive because of the Platonistic character (in Quine's sense) of classical mathematics. The existence of a model (interpretation) for a formal system is simply the existence of a mathematical configuration, and the proof of this existence need not be subjected to any special restriction.[5]

Between these two subjects Beth interposes a third, syntax, which refers only to the formal properties of the system but may use nonconstructive proofs. This has the mark of a hybrid, since the main new concept, that of a normal valuation (p. 263), differs from a model simply by the artificial device of replacing the truth values True and False by the numerical values 2 and 0. Hybrids are possible enough and may be fruitful, but this introduction of a hybrid without

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[3] Beth represents Gödel's theorem, which was proved constructively, as the result of a "policy of liberalization" admitting nonconstructive methods (p. 257).

[4] This is a minimal requirement. Hilbert's finitary methods are narrower, and it is possible to be narrower still.

[5] Beth implies (p. 251) that there is a clear finitary formulation of the completeness of elementary predicate logic, but that it is only "unlikely" (p. 252) that it can be *proved* by finitary methods. It seems to me, on the contrary, that there is a question as to the *formulation*, but with the formulations that come to hand, we know whether they are finitistically provable.

first clearly presenting its parents has the effect that Beth's treatment of basic logic is not very coherent.

We now turn to Beth's philosophical statements. He does not find the situation in general philosophy very favorable to an approach to the philosophy of mathematics from that side (p. 614). He does, however, derive two theses from general philosophy. The first is epistemological realism. The second is the denial of Brouwer's thesis that mathematics is independent of mathematical language, so that syntactic investigations are philosophically significant.

It is more interesting to ask what lessons he derives from the results of mathematical logic. The most important are those which imply essential limitations of formalized theories. The most important of these is Gödel's theorem, which implies that mathematics cannot be identified with a single formal system, and that the consistency of comprehensive formal systems cannot be shown by the most intuitively evident methods. Beth lays more stress on the "paradox of Skolem-Löwenheim," which is a consequence of the proofs of the completeness of elementary predicate logic. This is that systems, for example of set theory, in which the existence of nondenumerable sets can be proved, nevertheless have models which contain only a denumerable number of elements. And in general, logical systems of sufficiently rich vocabulary have "non-standard models," that is, interpretations which are incompatible with the intended one.

The conclusion which Beth draws is "that deductive theories cannot, in general, provide an adequate description of mathematical structures" (p. 643) and that the intuitive, immediate element in mathematical knowledge is irreducible. However, Beth is quite indefinite a to the character of this immediate element. It

> may be called either, according to Gonseth, a "*physique de l'objet quelconque*" or, in Brouwer's sense, a psychology of the creative subject. This ambiguity can be explained by reference to the parallelism between the physical and subjective spheres of reality [p. 645].

It seems to me that in order to justify accepting both Brouwer's and Gonseth's phrases, one would need an analysis of the "parallelism" of a type of which Kant's is an outstanding example. But elsewhere in the book, Beth shows little friendliness toward Kant.

In general, the philosophical comments in the book are marred by a lack of clear definition and close argument. For example, in the discussion of nominalism (ch. 16), there is vagueness as to what "Platonism" and "nominalism" are. In discussing Gödel's concept

of constructible set (section 129 and p. 473), he implies that these sets are a model for set theory which "serves the purpose of nominalism." He gives the impression that this is Gödel's view, although Gödel in effect denies that the model is nominalistic.[6] Beth also refers to Quine's "New Foundations" as a nominalistic system,[7] although it is certainly not so by Quine's own criterion, and Beth gives no other.

In summary, we say that this book contains a massive amount of logical material, not very well presented, and that it is philosophically somewhat thin. One might ask why a man with as many interests as Beth should go to the trouble of writing such an immense book, if the job is not to be done more thoughtfully.

CHARLES PARSONS

Harvard University

THE EDGE OF OBJECTIVITY: AN ESSAY IN THE HISTORY OF SCIENTIFIC IDEAS. By CHARLES COULSTON GILLISPIE. Princeton, Princeton University Press, 1960. Pp. x, 562. $7.50.

The history of science is entering on a new phase, and this book deserves to be read as one of the founding documents. Up until the early twentieth century, most historical studies of the natural sciences were written either by men actively working in science, whose retrospective vision was inevitably foreshortened, or by partisans of rationalism for whom the story of scientific progress was an improving tale with an anticlerical message. (A few distinguished Catholic scholars returned the compliment, but their works were little less partisan than those of their opponents.) The first man to bring to the history of science the patience, breadth, and bibliographical industry demanded by modern historians was the late George Sarton, whose monumental labors have placed all succeeding workers in his debt; but even he lacked a proper sense of proportion when it came to interpreting his material, and his enduring achievement is the vast collection of raw material he has bequeathed to us. The bricks for a true history of scientific ideas having been collected, the time has

[6] On pp. 146-147 of "Russell's Mathematical Logic," in P. A. Schilpp, ed., *The Philosophy of Bertrand Russell* (Evanston, Ill., 1944). The constructible sets are those obtained by iterating certain set-generating operations through all the ordinal numbers, and no nominalistic or conceptualistic way of justifying this iteration is either claimed by Gödel or given by Beth.

[7] *From a Logical Point of View* (Cambridge, Mass., 1953), ch. 5.

come for the architects to start work. And Professor Gillispie, whatever one says about the details of his design, certainly makes in this book a notable start on the new task.

Gillispie's canvas extends in time from Galileo to Einstein, in scope from the most abstract of physical theories to biology and geology —a subject on which he has already written another excellent book, *Genesis and Geology*. His method is that of the intellectual portrait: he takes the leading figures whose ideas have transformed our way of thinking about nature, reconstructs the quandaries in which they found themselves, and depicts— often in a wonderfully illuminating way—the ways in which they struggled to solve their problems, the hesitations which afflicted them, and the two-way interactions by which they influenced and were influenced by the intellectual climate of their times. Without the formal exercise of a philosophical method, Gillispie succeeds in producing portraits which professional philosophers will find of the greatest value, not only to their students but also to themselves. Consider, for instance, his analysis of Descartes: it would be difficult to write a better short account than Gillispie does of Descartes' central scientific ambitions, or to continue (after reading it) trying to expound his philosophy and epistemology without reference to these scientific motives. The famous "clarity and simplicity" at which Descartes aimed was to be the chief characteristic of an essentially scientific system of thought, and Gillispie does well to emphasize that the *Discourse on Method* was a preface "not to what science has become since Newton, but to what Descartes meant science to be"—the manifesto (one might say) of a scientific movement that never came off. This is only one example among many, in a book which should be rich in enlightenment for philosophers. Gillispie performs feats of almost equal brilliance in the cases of Galileo, Condillac, Lavoisier, Lamarck, Darwin, Helmholtz, Maxwell, and Mach, and the list could be continued. His concluding epilogue, especially the remarks about the relation between positivism in philosophy and energeticism in physics, deserve special mention. At every point, he has brought a powerful intellect to bear on the interpretation rather than the bare narration of the story he has to tell. One is not compelled to follow him all the way at every point, but one must respect the temperate and reflective judgment to which he has come from long study of his material.

In fact, Gillispie's book is held together by a central thesis which is implicit in his title and does something to shape his interpretations. The crucial thing about the modern scientific movement for him

is the pursuit of "objectivity," which means, to him, the solid intellectual security brought to empirical inquiry by the use of numerical measurement and mathematical analysis. True, he recognizes clearly that mathematics can enter into science in many ways. One finds in him no commitment to a naïve model of all mathematical sciences as "axiom systems." There is much to be said for this central doctrine; yet at times, one suspects, he takes it too far. For instance, Darwin's greatness is undeniable, so Gillispie is compelled to argue that Darwin's theory was a genuine instance of "objectivity" in his sense. Yet this involves stretching the facts somewhat, since the quantitative study of mechanisms of evolution is a feature of twentieth-century biology alone. Again, one of the noteworthy features of Gillispie's book is his account of the neo-Stoic strains in French eighteenth-century thought, notably in Lamarck. (Few people knew, before Gillispie pointed this out, that Lamarck's evolutionary zoology was only the special application of a general doctrine which he introduced first in physics and chemistry; the success of Lavoisier had completely eclipsed Lamarck's views.) Yet, while telling us very properly about Lamarck's chemistry, Gillispie dismisses it as unscientific—one more regrettable lapse from "objectivity," like Goethe's excursions into botany and optics and the work of the nineteenth-century German *Naturphilosophen*. At the present time, however, when the success of wave mechanics and the revival of speculation about the genesis of the chemical elements has brought back into physics something highly reminiscent of Stoic doctrine, one must ask whether Gillispie is entirely fair to Lamarck. The Stoic doctrine has survived for too long, and echoes too loudly today, to be dismissed as mere unscientific romanticism.

To sum up: this is a rich book, full of mature and lucid judgments vigorously expressed. In style it is generally distinguished and readable (the quadruple negative at the top of page 89 is one of a very few lapses). The book should be widely read by philosophers, historians, and scientists alike; and they should find plenty in it to talk about, not only among themselves, but also to each other. In a fragmented academic world, this is probably the book's greatest single merit.

STEPHEN TOULMIN

Nuffield Unit for the History of Ideas, London

COMMENTARY ON KANT'S CRITIQUE OF PRACTICAL REASON. By LEWIS WHITE BECK. Chicago, University of Chicago Press, 1960. Pp. xvi, 308. $6.00.

Fully aware of what he calls "the notorious acerbity of Kant-literature" Professor Beck has shown courage in producing the first full-scale commentary in English on Kant's neglected but controversial *Critique of Practical Reason.* Writing as one who is not an adherent of the school of Kantian interpretation with which Beck aligns himself, I should like to begin my necessarily brief remarks by expressing my unqualified admiration for Beck's achievement. The thoroughness in historical detail, the determination to face difficult points squarely, the depth of philosophical insight, and the clarity of exposition make Beck's *Commentary* an unquestionably first-rate piece of Kantian scholarship which ranks along with the great German, French, and British commentaries on Kant. The book will deservedly become a standard work of reference for many generations of Kantian students. The University of Chicago Press is to be complimented on matching the quality of the content of the book with an unusually high standard of printing and typography.

The *Commentary* is devided into three sections and is equipped with a comprehensive bibliography (the omission of Heinrich Barth's *Philosophie der praktischen Vernunft* is presumably an oversight) and adequate indexes. The first section of four chapters contains an excellent discussion of the general philosophical background and an account of the circumstances which led to the writing of the *Critique.* The claim made here, however, common to both Beck and Paton (to whom the work is dedicated), that "given the circumstances" Kant writes surprisingly well cannot be passed over in silence. Beck is surprised that two of the most important topics in the book are not even mentioned in the Preface and Introduction (p. 47); he accuses Kant of repeatedly ignoring the significant distinction between an imperative and a law (p. 84); he finds the second chapter of the *Analytic* cryptic, inept, and poorly constructed (p. 127); he finds the obscurity of the table of categories of freedom baffling (p. 144); he declares that Kant "wove a confusing fabric" about freedom (p. 176); "the usual high quality of Kant's workmanship" is asserted to be lacking in the *Antinomy* (p. 246). These and other similar critical comments are fully justified. If Kant's own gloomy estimate of his literary abilities had not been correct, then it might not have been necessary for Beck to claim (rightly) that "debates as to whether

Kant was right or wrong have been vitiated by lack of the most rudimentary agreement as to what he actually said and meant" (p. ix). Kant's greatness as a thinker is not a function of his literary ability.

The second section of eight chapters analyzes Kant's teaching in the *Analytic* with some references to the *Dialectic*. The parallel suggested with the metaphysical, transcendental, and subjective deductions of the first *Critique*, although possibly hazardous in detail, is enlightening and affords Beck the occasion for an admirable arrangement of his expository and critical material. Although Beck's discussion of freedom in Chapter IX does more to disentangle Kant's "confusing fabric" than any previous discussion known to me, his treatment of the moral law and the types of imperatives still seems to suffer from some of the obscurity of the original. While avoiding the fallacy of identifying the moral law with the categorical imperative, Beck none the less speaks of categorical imperatives as "telling a rational being" to act in a certain way. The metaphor here is deadly. A moral law, conceived of as a command, *may* be said to "tell" a person to do something, but it is difficult to see how an imperative which "indicates the relation of an objective law of reason to a will" (Kant) can either tell anyone anything or be obeyed (on page 73 Beck declares that Kant assumes that a person can obey a categorical imperative). This tendency to erect the imperative (originally the statement of a relation) into a command perhaps accounts for Beck's claim (p. 86) that it is not clear in what sense an imperative is the kind of judgment which could be analytic or synthetic. Kant made this quite clear in a key footnote to Section II of the *Foundations*.

The third section of two chapters discusses the *Dialectic*. The brevity of this section is due to Beck's conviction that the central antinomy "is really quite a poor thing" unable to bear the weight placed on it by Kant. Holding that "the command to seek the *summum bonum* is merely the command to do my duty" Beck does not find a close connection between the moral law and the postulates of practical reason. Waiving the problems inherent in the use of the word "command," I should contend that Kant would have said that the command to seek the *summum bonum* is the command to do my duty, and this is within my power. If Beck's claim that "I can do absolutely nothing else [than act out of respect for the law] towards apportioning happiness in accordance with desert" were true, then his criticism of the Kantian position might stand. Holding that on occasion we can, and that where possible we ought to, apportion happiness in accordance

with desert (in agreement, for example, with W. D. Ross), I seem to detect rather more force in Kant's admittedly poorly stated antinomy than does Beck. My only serious complaint about the *Commentary* is that, perhaps misled by Kant's inability to express himself clearly and unambiguously, Beck did not devote more space to discussing the problem of the relation between happiness and morality, a problem which so worried moralists of such apparently different persuasions as Kant and Henry Sidgwick. The Kantian architectonic hinders the clear exposition of what is surely the most profound and difficult problem in moral philosophy.

Any acerbity in the above paragraphs, the sympton of an occupational disease, must be considered as directed toward Kant. Beck's *Commentary* I have read and shall continue to read with pleasure and profit.

A. R. C. DUNCAN

Queen's University, Kingston, Ontario

THE IMAGINATION AS A MEANS OF GRACE. By ERNEST LEE TUVESON. Berkeley and Los Angeles, University of California Press, 1960. Pp. 218. $5.00.

This book traces the influence of John Locke's view of the nature of the understanding on subsequent aesthetic theory in Great Britain. Instead of the earlier unified picture of human reason, in which art was a means of knowledge and beauty something known by reason, Locke's limitation of the powers of the understanding effected a fragmentation of the older reason into separate faculties. Those functions of reason which Locke excluded from the understanding were allotted to other parts of the mind. In time, these subdivisions of reason—understanding, moral sense, imagination—ceased to be regarded as equally rational. To be rational becomes the prerogative of the understanding while imagination loses its cognitive character without, however, thereby losing its value. Thus to lack strict rationality is no longer to lack intellectual respectability. Art is no longer seen as a way of communicating truth and becomes instead something which stirs the emotions, produces pleasure, liberates the soul, or appeals to the unconscious.

In order to show the course taken by aesthetic theory in Britain, Tuveson selects for intensive consideration and comment a few writers whose views he thinks typical and instructive. These range from the celebrated (but, Tuveson suggests, not sufficiently respected) Addison

to the relatively unknown Mrs. Elizabeth Montagu. In tracing Locke's influence on these writers, Tuveson is not interested in showing how their views logically entailed Locke's epistemology nor in pointing to explicit acknowledgments of the influence of Locke. Tuveson's method raises a general puzzle about the history of ideas; and his book is a history of ideas, not a philosophical work. History is a mixture of the chronicle (first this, then that) and the causal sequence. But when the history is that of ideas, then it seems to be thought that what relations there are, in addition to the merely temporal ones, between earlier idea *A* and later idea *B*, must be not merely causal but logical. Since it is ideas which are causes, their effects must be to some extent their logical consequences. Thus Mr. Tuveson never tries to establish such plain historical theses as that Mrs. Montagu or Elizabeth Carter had read Locke or his disciples. He rather argues in a quite a priori way that what they wrote is what one would expect to be written after Locke. One is, however, to expect this not because it directly presupposes Locke's doctrines, but in virtue of some a priori psychological compulsion which the earlier ideas exert and which allows them to have as effects mutually contradictory doctrines.

This uneasy middle way between philosophical analysis and historical research is one whose discipline is hard to discern and of whose merits I am doubtful. Not that Tuveson's book is dull or unperceptive. On the contrary, there are illuminating discussions of individual writers and, in particular, of Addison. It is the general method which produces little conviction in the reader. I felt that from another selection of writers, or from another interpretation of these same writers, a different pattern of development could equally plausibly be exhibited. All historians must select and interpret events, and so the patterns they trace are to some extent arbitrary, but historians are usually more cautious than Tuveson in imputing initial causal responsibility for the developmental patterns they find. To make Locke responsible for a motley sequence of doctrines which range from a hedonist aesthetic through various forms of romanticism to art for art's sake, symbolism, and on to a Freudian interpretation of art, seems at first sight far-fetched. At the end of Tuveson's attempt to make it seem a likely tale one is left still skeptical of paternity claims which cover such varied progeny.

A. C. Baier

BOOKS RECEIVED

L'etica di John Dewey. By Adriano Bausola. Milano, Vita E. Pensiero, 1960. Pp. 225. L. 2000.

Crime in America: Controversial Issues in Twentieth Century Criminology. Edited with an introduction and critical comments by Herbert A. Bloch. New York, Philosophical Library, 1961. Pp. 355. $6.00.

A History of Formal Logic. By I. M. Bochenski. Translated and edited by Ivo Thomas, Notre Dame, University of Notre Dame Press, 1961. Pp. xxii, 567. $20.00.

La estructura de la obra literaria (Una investigación de filosofia del lengua je y estética). By Félix Martínez Bonati. Santiago, Universidad de Chile, 1960. Pp. 171.

Plato on the One: The Hypotheses in the PARMENIDES. By Robert S. Brumbaugh. New Haven, Yale University Press, 1961. Pp. xv, 364. $6.50.

The Logic of the Humanities. By Ernst Cassirer. Translated by Clarence Smith Howe. New Haven, Yale University Press, 1961. Pp. xx, 217. $4.50.

L'ultimo Heidegger. By Pietro Chiodi. Torino, Taylor, 1960. Pp. 138. L. 650.

Inductive Probability. By John Patrick Day. New York, Humanities Press, 1961. Pp. xvi, 336. $8.00.

Metaphysics and Historicity. The Aquinas Lecture, 1961. By Emil L. Fackenheim. Milwaukee, Marquette University Press, 1961. Pp. x, 100. $2.00.

Current Issues in the Philosophy of Science. Symposia of Scientists and Philosophers. (Proceedings of Section L of the American Association for the Advancement of Science, 1959.) Edited by Herbert Feigl and Grover Maxwell. New York, Holt, Rinehart and Winston, 1961. Pp. xii, 484. $6.00.

Platon. Vol. III, *Die Platonischen Schriften zweite und dritte Periode.* By Paul Friedländer. Berlin, Walter de Gruyter & Co., 1960. Pp. vi, 534. DM 38.

Reason and Revelation in the Middle Ages. By Etienne Gilson. New York, Charles Scribner's Sons, 1961. Pp. 114. $1.25 (paper).

Feeling and Expression. An Inaugural Lecture delivered at University College, London, 25 October 1960. By Stuart Hampshire. London, H. K. Lewis & Co., Ltd., 1961. Pp. 22. 2*s.* 6*d.*

Philosophy and the American Heritage. By Johnson D. Hill and W. E. Stuermann. New York, Philosophical Library, 1961. Pp. viii, 254. $3.50.

The Foundation of Jacques Maritain's Political Philosophy. By Hwa Yol Jung. Gainesville, University of Florida Press, 1960. Pp. 66. $2.00.

Über die Leibnizsche Logik. Mit besonderer Berucksichtigung des Problems der Intension und der Extension. By Raili Kauppi. Helsinki, Societas Philosophica, 1960. Pp. 279.

Critique et Moral chez Kant: Philosophie und Moral in der Kantischen Kritik. By Gerhard Krüger. Translated by M. Regneir with a preface by d'Eric Weil. Paris, Beauchesne, 1961. Pp. 275. 15 NF.

An Introduction to General Metaphysics. By Gottfried Martin. Translated by Eva Schaper and Ivor Leclerc with a foreword by Sir David Ross. London, George Allen & Unwin Ltd., 1961, Pp. 156. 18*s.*

A Study in Wittgenstein's Tractatus. By Alexander Maslow. Berkeley and Los Angeles, University of California Press, 1961. Pp. xxii, 162. $4.00.

Philosophical Aspects of Culture. By Bertram Morris. Yellow Springs, Antioch Press, 1961. Pp. xvi, 302. $7.00.

Free-Will and Determinism. By Allan M. Munn. Toronto, University of Toronto Press, 1961. Pp. 218. $6.00.

Systematic Pluralism: A Study in Metaphysics. By Henry Alonzo Myers. Ithaca, Cornell University Press, 1961. Pp. viii, 195. $3.75.

Moral Man and Immoral Society: A Study in Ethics and Politics. By Reinhold Niebuhr. New York, Charles Scribner's Sons, 1961. Pp. xxv, 284. $1.45 (paper).

John Stuart Mill and the Harriet Taylor Myth. By H. O. Pappe. Melbourne University Press; New York, Cambridge University Press, 1961. Pp. ix, 51. $2.00.

The Naturalistic Tradition in Indian Thought. By Dale Riepe. Seattle, University of Washington Press, 1961. Pp. xi, 308. $5.00.

A Critical Survey of Indian Philosophy. By Chandradhar Sharma. New York, Humanities Press, 1961. Pp. 415. $6.00.

A Whiteheadian Aesthetic: Some Implications of Whitehead's Metaphysical Speculation. By Donald W. Sherburne. New Haven, Yale University Press, 1961. Pp. xxx, 219. $5.00.

Modes of Referring and the Problem of Universals. By D. S. Shwayder. Berkeley, University of California Press, 1961. Pp. x, 164. $3.50.

Morale. By René Simon. Paris, Beauchesne, 1961. Pp. 290. 15 NF.

The Philosophy of Being: Metaphysics I. By Gerard Smith and Lottie H. Kendzierski. New York, Macmillan Co., 1961. Pp. xvii, 408. $5.75.

The Philosophy of Physics. Edited by Vincent Edward Smith. Jamaica, St. John's University Pres, 1961. Pp. 85. $2.50.

The Phenomenological Movement: A Historical Introduction. By Herbert Spiegelberg. 2 volumes. The Hague, Martinus Nijhoff, 1960. Pp. xxxii, 391; xi, 393-735. 47.50 guilders.

Principles of Cartesian Philosophy. By Baruch Spinoza. Newly translated from the Latin by Harry E. Wedeck, with a preface by Dagobert D. Runes. New York, Philosophical Library, 1961. Pp. 192. $4.75.

What is Philosophy? A Short Introduction. By Elmer Sprague. New York, Oxford University Press, 1961. Pp. xii, 139. $1.60 (paper).

Spinoza in the Light of the Vedānta. By Rama Kanta Tripathi. Banaras, Banaras Hindu University, 1957. Pp. xvi, 349. Rs. 12.

Hume, Precursor of Modern Empiricism: An Analysis of His Opinions on Meaning, Metaphysics, Logic and Mathematics. By Farhang Zabeeh. The Hague, Martinus Nijhoff, 1960. Pp. viii, 166. 12.50 guilders.

Ye Shall Know the Truth. By Harold W. G. Allen. New York, Vantage Press, 1961. Pp. 241. $3.95.

Logic: The Theory of Formal Inference. By Alice Ambroce and Morris Lazerowitz. New York, Holt, Rinehart and Winston, 1961. Pp. viii, 78. $2.00.

Anales del Instituto de Investigaciones Psicopedagogicas, Vol. V. San Luis, Argentina, Facultad de Ciencias, Universidad Nacional de Cuyo, 1957-1958. Pp. 319.

Surrealism: The Road to the Absolute. By Anna Balakian. New York, Noonday Press, 1961. Pp. x, 209. $1.45 (paper).

Great Western Mystics: Their Lasting Significance. By David Baumgardt. New York, Columbia University Press, 1961. Pp. xvi, 99. $3.00.

Contemporary European Philosophy. By I. M. Bochenski. Translated from the German by Donald Nicholl and Karl Aschenbrenner. Berkeley, University of California Press, 1961. Pp. xviii, 326. $ 1.75 (paper).

An American Dialogue: A Protestant Looks at Catholicism and a Catholic Looks at Protestantism. By Robert McAfee Brown and Gustave Weigel. Garden City, Doubleday & Co., 1961. Pp. 240. $.95 (paper).

Osiris: The Egyptian Religion of Resurrection. By E. A. Wallis Budge. New Hyde Park, University Books, 1961. Pp. xliii, 440. $15.00.

Religion in American Life. Vol. IV, Parts 1-5 (2 volumes), *A Critical Bibliography of Religion in America.* By Nelson R. Burr. In collaboration with the editors, James Ward Smith and A. Leland Jamison. Princeton, Princeton University Press, 1961. Pp. xx, 541; xv, 542-1219. $17.50.

Christianity and History. By H. Butterfield. New York, Charles Scribner's Sons, 1961. Pp. viii, 146. $1.25 (paper).

Leibniz. By Herbert Wildon Carr. New York, Dover Publications, 1961. Pp. vi, 222. $1.35 (paper).

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